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EDITORIAL.

ITEMS AND NOVELTIES.

The Church Street District, Boston, Massachusetts.— An extensive engineering work, in change of grade with raising of buildings, and street widening by moving them, is now in progress at Boston.

To give a proper understanding of the operations, we will state that the present site of the city of Boston was originally four small hills, which in very high tides became islands, but at mean tides were so connected by marshes that the whole became a peninsula, of irregular shape, about three-fourths of a mile in greatest width, and one and three-fourths of a mile long, with a neck about one and a fourth miles long connecting to the main land. From the three higher hills, Copps, Beacon and Fort hills, Boston was called tri-mountain, the fourth, Holles hill, being only a knoll of perhaps twenty-four feet summit elevation above tides. Copps hill was the earliest set-

tled. Its crest has been preserved by a grave-yard and fort (now all grave-yard), and the eastern side of it was, twenty years since, an almost exact miniature copy of the old part of London city, having been built in the same manner and at the same time, (that is, after the great fire at London,) with streets, alleys, styles of construction, in careful similitude to the great city. The past twenty years has demolished these remnants of the seventeenth century more completely than they have been removed in London. So that the old inhabitant of Boston can find Queen Ann street, Moon street, North Square, readily, in London, to-day, while he could hardly find a recollection of these localities at home.

Beacon hill and Fort hill were country residences. The former was encroached upon for city use about the beginning of this century, and the summit, some twenty feet in height, removed; and later, about forty years since, extensive excavations graded the eastern slope. It is yet about sixty to eighty feet elevation at the State House. Fort Hill was also covered with dwelling houses thirty to fifty years since, which have in course of time been isolated from the other dwelling house parts of the city by the increase of warehouses, and have degenerated into tenements for the poorer of the laboring population, mostly Irish.

Now, a cut, eighty feet wide by six hundred feet long, and sixty feet in greatest depth, has ruthlessly opened the narrow lanes and winding ways, and encroached on the aristocratic seclusion of Washington Square, with its circular green (the old Fort), which once gave the most beautiful prospect of the country and harbor. A few years more, and this landmark will disappear.

But to return to our subject. Amongst the improvements of Boston of fifty years ago was a mill dam, about one and a fourth miles long, enclosing some two square miles of tidal basin, or rather two basins, as a cross dam of three-fourths of a mile in length separated the two. One of these basins was intended for high water and the other for low water, and as a constant difference of about six feet could be maintained in the water level of these two basins, it was supposed a very valuable water power would be established.

Alas! Pennsylvania and Pictou coal proved too strong a rival to the water power, and, as a commercial enterprise, it failed. But after many years, the growth of Boston, far exceeding the limits of ground, has brought this property into the market as land, and those speculators or investers who have kept their property in the

Boston Water Power Company now find all their anticipations of returns far more than realized in the value of land for building purposes, or rather in the value of that share of the land which, with the State and other claimants by rights in the shore, has been divided; after thirty feet deep of gravel has been brought by railway, and deposited on four or five square miles of marsh and basin. About one square mile is already filled in.

This filling and extension of the city to the westward has involved the considerations of drainage, level, and grades, which has not been the least of the troubles to provide for.

Along the western and southwestern edge of Holles hill, a street, Pleasant street, was at the time of the Revolution laid out and occupied, and the high tides came up to the back end of the lots on this street. After the formation of the low water basin by the Boston Water Power Company, the level of tidal flow was here depressed some nine to eleven feet, and upon the slope of ground, about nine hundred feet long by six hundred feet wide, were laid out several narrow streets, Church, Marion, Fayette, South Cedar and others, which were built up with small houses, much like those of Southwark, London, or Southwark, Philadelphia. None of the houses rose above three low stories in height, most of them but two, with or without a basement half story. The only large building was a Methodist church, from which Church street derives its name. The streets were none over thirty feet in width, and mostly twenty or twenty-five feet, and some lanes of twelve or sixteen feet. This little tract of territory, consequently, about equal in size to three Philadelphia squares, and having exactly the same number of houses that are placed upon three Philadelphia squares, that is, four hundred and fifty, became, by the raising of the ground beyond it, a sunken spot about twelve feet on the average below the possibility of drainage, with about seventeen and a half feet depression at the lowest point.

After much discussion, the city determined to raise the entire district as a whole, and to take advantage of the disturbances of *immovable* property at the same time, by widening all the narrow streets, so that none should be less than twenty-five or thirty feet between the houses.

Operations were commenced about the first of July last, but it was not until about the first of August that direct work on the district was begun. The number of buildings to be raised—some of them

to be moved, also—was four hundred and fifty, or about that number. The commissioners immediately circumscribed rather more than three-fourths of all the district and placed it under contract. This portion of the district consisted of all south of Church street, and that between Church and Pleasant streets east of Marion street, thus leaving only a comparatively small piece bounded by Church street, Marion street, Pleasant street and Madison place. When fairly begun, the work was pushed with the utmost vigor, and surprising progress was the consequence. The spot chosen for beginning was at the foot of Fayette street, which was the most depressed place on the territory. Here the buildings were soon raised, and the cars, running along the temporary track in Ferdinand street night after night, deposited their loads of gravel, to be shovelled or carried in wheelbarrows under the stilted houses during the day. This was the beginning in the first part of August. Now every building on the division described as being under contract is raised or being raised. The filling has progressed equally well. The tracks of the temporary railroad are laid in Grenville place, Ferdinand street, and Fayette street. All night the trains are running, and the gravel is dumped off at the sides of the embankment in the streets on which the tracks are laid. The number of car loads brought in each night varies between two hundred and two hundred and fifty, and as two car loads make a square of filling (eight cubic yards), from one hundred to one hundred and twenty-five squares are thus added nightly to the territory. In vacant places between Church, Ferdinand, South Cedar and Marion streets, and between Berlin, Shawmut, Church and Pleasant streets, there are now large surplus deposits awaiting the completion of the raising of the buildings surrounding, when the gravel will be spread. As soon as a building is ready, the laborers, who number about one hundred in this department, convey the gravel from the track sides underneath the house, which is soon standing on the new *terra firma*. In this way the work has been done, and the result of about four months' labor is the filling of about one-third of the entire district. The only streets on which no filling has been done are Piedmont and Marion, and these will very soon be reached.

In South Cedar street hand cars are now employed, and it is intended to run the locomotives and trains there presently. It will be readily seen that the progress in future will be much more rapid than that heretofore, in consequence of the increase of the facilities

for employing the gravel trains. At the close of the season, or when further operations will be prevented by the extreme cold weather, three-fourths of the entire work will be completed; and this will have been effected in about five months, and at an expense safely inside the estimates. There have been no serious obstacles, and the course of the commissioners has been unexpectedly smooth. The outlay up to the 1st of November had been $200,000.

About six houses, or one hundred and twenty feet long, are usually raised in one block together, the walls, front and back, being cut, or rather a line of separation being made at about each sixth house. The raising is done by screws, as usual, and the blocking is the usual cob-house work of pieces of timber. The moving is on wooden rollers, round iron bars, and on balls, with no apparent reason for choosing or using either. Of course, those houses at the ends of each block, in raising, where the party wall is removed, are carefully clamped and braced. And about every fourth house has been found to require the front and back walls to be clamped by through bolts to the floors. The walls may be said to be universally but eight inches—one brick thick.

About one-half the houses are occupied, the raising or moving not interfering with the domestic comforts of the household, although it would seem inconvenient to small children to find the doors seventeen feet above ground, and it is possible that the ladies object to the ladder ingress and egress. From an examination of the buildings, very few cracks are discernible, either in those which have been finally underpinned, or in those undergoing the transitions. The ground is fortunately all solid, (not made, as the Boston name goes,) and the underpinning is either done in brick or in cubical granite blocks, which lay without the settlement of what is generally known as rubble masonry.

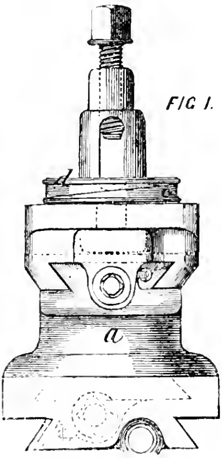
This instance of moving brick edifices has not the extent of area, as a whole, or the magnitude of buildings which the Chicago example presented, but the work going on at one place is large, and the numerous walls, the narrow passages, the height of elevation, and the moving of the houses, makes it, in detail and labor, noteworthy beside that memorable instance of engineering. B.

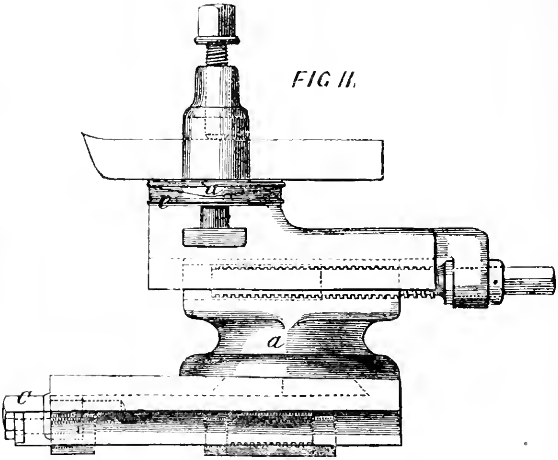
Tool Rests.—The accompanying illustrations represent various forms of tool rests employed by Messrs. Bement & Dougherty on their lathes.

We will first describe the older form, and then point out the modifications which have been introduced, and are found applicable, with advantage, to many cases, without displacing the other forms which are still largely employed.

Fig. 1 is an end, and Fig. 2 a side elevation of an adjustable rest with compound traverse, which has two noticeable peculiarities, and which has been long in use.

In the first place, the mode of adjusting the tool by means of two case-hardened, wrought iron rings, or circular inclined planes, with milled edges, encircling the tool-post at *d*. By placing the thick edge of one and the thin end of the other together, the upper surface is made horizontal, and by placing the two thick edges together the greatest angle of adjustment is obtained. Between these limits any degree may be got by turning one or both the rings, as the case may require.





The other peculiar feature is the mode of swivelling and clamping the upper portion, *a*, of the rest upon the lower. This is done by a circular, *v*, as shown in dotted lines, having, on one side, a certain portion cut out, into which is fitted a tail-piece, *b*, worked by a set screw, *c*. The effect of this, when clamped tightly by the screw, is to wedge the upper portion, *a*, down hard against the lower one, and thus secure great rigidity.

The new style of construction is shown in Plate I., where Fig. 1 represents an adjustable rest intended for lathes where the ordinary single traverse movement suffices for the requirements of the work.

ADJUSTABLE TOOL REST,
by
Bement & Dougherty,
Industrial Works,
Philadelphia, March 1868.

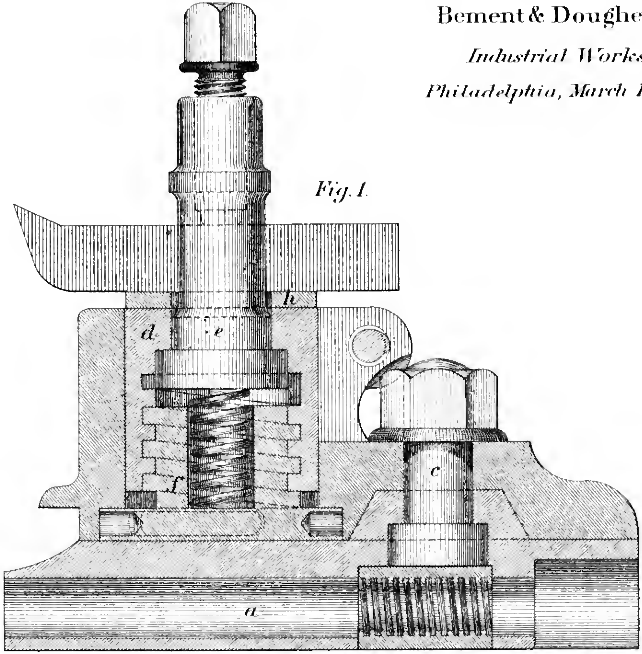


Fig. 1.

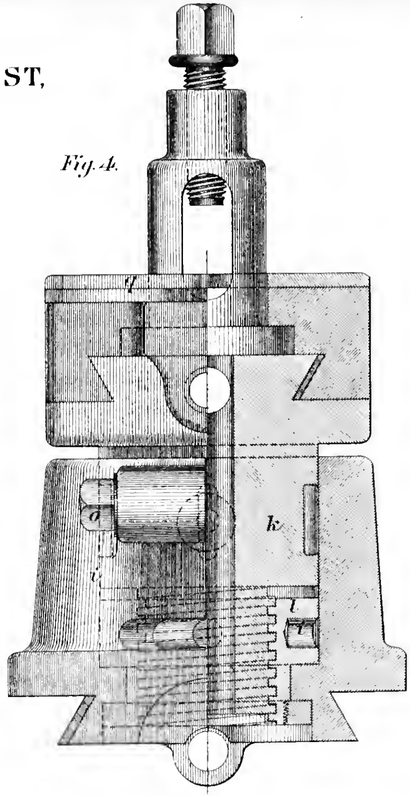


Fig. 4.

Lith. Jas. Mc Guigan, Philada.

a is the usual slide worked by a screw in the lathe-carriage; *b* is the main or lower portion of tool rest swivelling round the clamping-bolt, *c*. The tool-post, of the usual shape, is held in a nut, *d*, into which works a screw, *f*, and raises or lowers the nut, *d*, with its tool-holder. This screw is operated by the workman, with a small lever fitting into holes or sockets cut in the screw, as shown in Fig. 1.

A spline and feather in *b* keeps the nut from turning, and a bolt at *i* serves to clamp it in any position.

The advantage of this rest is, that the tool can, with the greatest convenience and accuracy, be raised and lowered after it is screwed up in its holder, or without slacking the set-screw.

Fig. 2, Plate I., shows a half end elevation and half cross-section of the same kind of rest with double traverse for lathes where universal adjustment is required. The upper slide, of the usual construction, has a column, *k*, on its under side, which is clamped into the other portion by the bolt, *o*, in the same manner as in Fig. 1; its lower extremity is a screw, upon which works the nut, *l*, operated by a lever in the manner already described in Fig. 1. This nut is stationary, and being rotated by the lever, raises and lowers the screw on *k*, carrying with it the tool-post and upper slide.

This rest can, with perfect facility and accuracy, be raised and lowered, and also swivelled into any position without unscrewing the tool. A small pin, dotted at *m*, works, by means of a spring, into slots in *k*, shown at *n*, which keep the rest square when it is desired to do so.

At *j* is seen the opening in the outer shell, through which the workman operates with the lever the internal nut or screw.

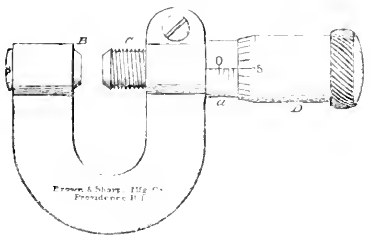
The whole makes a very desirable arrangement where accuracy of adjustment and convenience of working are required.

Pocket Sheet-metal Gauge, by Brown & Sharpe Manufacturing Company. At the last meeting of the Institute there was exhibited a new form of gauge, constructed by the firm above mentioned.

It involves the same essential advantages as are found in the larger sheet-metal gauges manufactured by the same establishment, and before described in this *Journal*. That is to say, it can take its measurement not from the edge of the sheet only (where irregularities are likely to occur), but at some little distance within. It also not only indicates whether the sheet in question does or does

not conform approximately to some one of a series of arbitrary standard measurements, but in every case gives the exact thickness in parts of an inch, reaching down to thousandths.

The accompanying cut shows the instrument of full size, and it may be described as follows:



The piece in the form of the letter U has a projecting hub, *a*, on one end. Through the two ends are tapped holes, in one of which is the adjusting screw, B, and in the other the gauge screw, C. Attached to the screw, C, is a thimble, D, which fits over the exterior of the hub, *a*. The end of this thimble is beveled, and the beveled edge graduated into twenty-five parts, and figured, 0, 5, 10, 15, 20. A line of graduations 40 to the inch is also made upon the outside of the hub, *a*, the line of these divisions running parallel with the centre of the screw, C, while the graduations on the thimble are circular. The pitch of the screw, C, being 40 to the inch, one revolution of the thimble opens the gauge $\frac{1}{40}$ or $\frac{25}{1000}$ of an inch. The divisions on the thimble are then read off for any additional part of a revolution of the thimble, and the number of such divisions are added to the turn or turns already made by the thimble, allowing $\frac{25}{1000}$ for each graduation on the hub, *a*. For example, suppose the thimble to have made four revolutions and one-fifth. It will then be noticed that the beveled edge has passed four of the graduations on the hub, *a*, and opposite the line of graduation will be found on the thimble the line marked 5. Add this number to the amount of the four graduations, which is $\frac{100}{1000}$, and it equals $\frac{105}{1000}$, which is the measurement shown by the gauge,

On the above occasion, Mr. Coleman Sellers mentioned that he had been using one of these gauges for several weeks, and had compared it with other standards by means of the set of tables furnished by the manufacturers, and that this test had proved it to be entirely reliable.

The Hoosac Tunnel.—The work at this tunnel having now reached such a point that all serious difficulties being surmounted, what remains to be done is simply a matter of removing so many yards of rock under fixed conditions, the further prosecution of the structure has been placed under contract to responsible parties, and

Mr. Latrobe, considering the continuance of his office as Consulting Engineer unnecessary, has resigned it.

Electric Exploder, for Blasting, by Charles T. Chester, Esq., of New York.—At the last meeting of the Institute, there was exhibited a very efficient apparatus, devised and constructed by Mr. Chester, for use in connection with mining, tunneling, and other like operations. It consisted of a cylindrical case of hard rubber, 4½ inches in diameter, and 10 inches long, with a crank and handle projecting from one end, as also a small button or knob; while from the other end stood out two rounded binding screws. The charge or percussion fuse being connected with the binding screws by insulated wires, the handle is turned round a dozen or twenty times, and then the button is pulled out about one inch, when a spark passes and fires the fuse.

The interior structure of the apparatus is as follows: At the axis of the cylindrical case is a small cylinder of hard rubber, rotated by the projecting handle. This has on one side a rubber covered with mosaic gold, and on the other a series of points, like an ordinary electrical machine. The rubber is connected with a sheet of tinfoil rolled within the case, and the points with another sheet face to face with the first, but carefully insulated from it. The apparatus is thus both a frictional electrical machine and a leyden jar. When the button is pulled out, two brass strips are caused to connect the binding screws with these two sheets of foil, and a discharge of course occurs through the fuse.

The advantages of this machine are its compactness and certainty. Being entirely enclosed in an insulating case, it is free from all disturbance on account of dampness, and has sufficient force to fire many hundred blasts at once, if required.

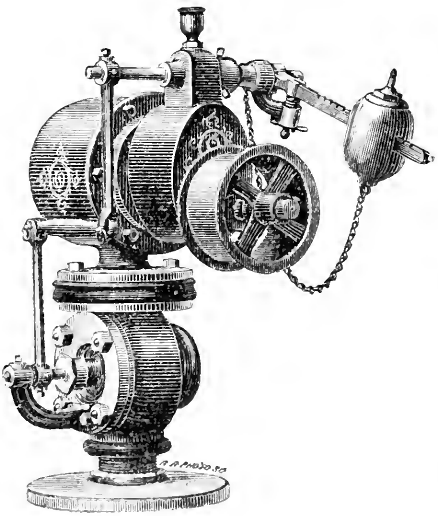
Our readers may remember that in a notice of the Hoosac Tunnel, in Vol. LIV., p. 9, we described, from a letter by Mr. E. S. Ritchie, the machine which has been in use there for several years, which was like this, in material and combination of leyden jar with the machine, but wanted the important feature of compactness and airtight inclosure. Its success, however, is an excellent guarantee for this.

Flow of Water in Rivers.—During the past two years, observations have been made, under the direction of the Superintendent of the Lake Survey, Gen. W. F. Reynolds, upon the flow of water in the several rivers which connect the several lakes, including St.

Lawrence, with a view to settle certain questions upon subaqueous soundings and hidden outlets. The river gauging has, from the start, been entrusted to Assistant Engineer Farrand Henry, who has conducted the work with much care and skill. He devised a "Telegraphic Current Meter," which is said to be more delicate and perfect than anything of the kind heretofore used, and hence the results will be of much value. He has not completed the reduction of all the observations, but the following are approximate results of last year's work:

RIVERS.	Maximum Velocity. Miles per hour.	Mean Velocity. Miles per hour.	Discharge cubic feet per second.
St. Marie	1·30	0·66	90·783
St. Clair	3·09	2·39	233·726
Detroit	2·71	2·04	236,000
Niagara	2·32	1·54	242·494
St. Lawrence	1·00	0·65	319·943

The Huntoon Governor.—At the last meeting of the Franklin Institute, there was exhibited and explained the steam and water-power governor above named.



This instrument consists essentially of a small propeller-wheel, driven by appropriate gearing from the machine to be regulated, and inclosed in a cylinder filled with oil, which is forced to one end by the action of the propeller, and allowed to flow back again by a controllable orifice. If a greater than the normal speed is imparted to the propeller, it will force forward the oil more rapidly than the returning orifice allows for, and thus the propeller will be forced back, notwithstanding the resistance of a weight which is graduated

to keep it up to its position, under the conditions of velocity and resistance. With a less speed, the actions are, of course, reversed.

Among the merits claimed for this improvement, is the ease and quickness with which the apparatus can be adjusted, so as to cause the engine to run faster or slower, by the simple arrangement of weights on the balancing lever.

2d. The perfect independence of its operation as regards the amount of opening to the valve; the velocity alone controlling the motion of this valve, and that equally, whether wide open or nearly shut. This not being the case with the centrifugal governor.

In that instrument, a certain position of the balls means a certain amount of valve opening, and if a sudden demand is made for more power, that is more steam, the balls must fall to obtain a greater opening of valve, but these can only fall when their speed slackens.

That instrument, therefore, cannot, without sensible change of velocity, regulate the admission of large and small quantities of steam.

Special automatic adjustments have been provided to meet this difficulty, but they can only produce their effects after several revolutions, and thus the instrument can never be delicate in its adjustment to meet sudden changes.

In the Huntoon Governor, however, if more steam is suddenly required, the valve may be opened to its fullest extent, in consequence of a loss of velocity in the propeller practically inapplicable, and if the supply of steam thus obtained proves sufficient, and the normal velocity is restored, the regulation goes on, with the valve thus widely opened exactly as before. This instrument thus secures what we might call a practically absolute velocity, in the presence of the greatest irregularity in the power called for, and produced.

We have had an opportunity of examining one of these governors in operation, attached to the engine driving the works of Messrs. William Sellers & Co., 1600 Hamilton Street, of this city. The governor which this instrument displaced was one of the ordinary ball governors; with it the engine ran over speed at high pressures of steam, and under speed at low pressures; in fact, it could not be run with steam much below 60 pounds pressure. This engine has 16 inches diameter of cylinder; 3 feet stroke and makes 50 revolutions per minute, with cut-off and lap of valve at about $\frac{5}{8}$ stroke; but of this we are not sure. Indicator cards showed 42

horse-power; the work required of the engine being very constant and regular. The Huntoon governor was adjusted to run the engine at speed with all the work off; the speed, when doing full work, was found to be the same: the noticeable difference in speed being at low pressure. Thus, at 50 pounds boiler-pressure, the engine runs a little over speed. This is accounted for by the portion of the counter-weight arm decreasing the pressure on the propeller. Cards taken rapidly in succession, show a great variation, indicating all the way from 52 horse-power down to 38 horse-power, the average being as before, 42 horse-power. The sensitiveness of the governor causing it to act as a cut-off. The parties using it seem well pleased with its action; and, indeed, numerous testimonials assure us of its efficiency and value, in all establishments where uniform velocity in the motive power is important.

Editorial Correspondence.

CORNISH PUMPING-ENGINES.

To the Editor of the Journal of the Franklin Institute:

THE remarks of your two correspondents who have been discussing the question of relative merits of the Cornish and Fly-wheel systems of Pumping-engines, do not appear to me to present to your readers the distinctive differences of the two methods of transforming the largely variable power of expansive steam to the impelling of the almost invariable load of a water column, and I send these few observations in hope that the grounds on which these differences are based may be more fully comprehended.

In the Cornish engine we have the application of steam to a heavy mass, which mass is taken from a state of rest, put into accelerating velocity by constant pressure, until the point of cutting off is attained, and is continued in varient velocity during the expansion of the steam until (if the engine is working with its best effect) the mass comes to rest again at the instant of release of the expanding steam. The mass thus acted upon is composed of two unequal parts, placed by some system of leverage so as to act in

opposite directions; the excess of weight which one of these parts has over the other, being the effective motor or load of the engine. The resistance of the air to the changes of velocity of the mass, and the resistance from friction of the machine construction, is comparatively small. The ratio which the excess of weight (of one part over the other, or load) bears to the total weight of the mass, determines the rate of expansion practicable.

In order to start the mass into motion, it is evident that the pressure of steam must have exceeded the (excess of weight of one part of the mass over the other) load; but once started, the motion ceases only (under the conditions stated) when the momentum is expended, and when all additional force, however small, has been absorbed. And the steam will have raised the load to the highest point possible.

This load is now applied to a column of water which, including a resistance due to some small velocity, it balances. The water column commences motion with very gradual acceleration, until at some point the velocity resistance balances the excess of the weight, and the motion then continues through the remainder of the return stroke, at the lowest uniform velocity which the relative conditions of weight to water establish, finally coming to rest upon a cushion of steam which it compresses to form the starting force of the load upwards once more. The utmost effective impulse is thus obtained from the weight. If the boilers are all they should be, (that is, if they will make an average of 12 pounds of evaporation per pound of combustible,) if the steam is carried to 60 or 70 pounds, the stroke be long, the cylinder well jacketed with live steam, the vacuum about 26 inches, the expansion about one-sixteenth to one-eighteenth, and the weight properly proportional to the work, a result of 130 millions will be ensured. That is, 130 millions of pounds of water will be lifted *one* foot high by one pound of coal.

In the Fly-wheel system of Pumping engine, up to and including the fly-wheel, the same efficiency of transformation of force exists as does up to and including the mass in the Cornish engine. No matter how large or how small an impetus is transmitted by the crank, from any variation of expansion or any condition of motion which the crank itself presents, that impetus is imparted to the moving fly-wheel in its fullest value. The friction resistances of the two engines may vary to some small extent, but these are not the real

differences of effect which are worthy of discussion or consideration. Given any Cornish engine lifting its weight, and any rotative one impelling its fly-wheel, and they can be so constructed that the frictions will be identical.

Consequently, there remains only the comparison of the transmission of power from a weight to a column of water with that from a fly-wheel to the same column.

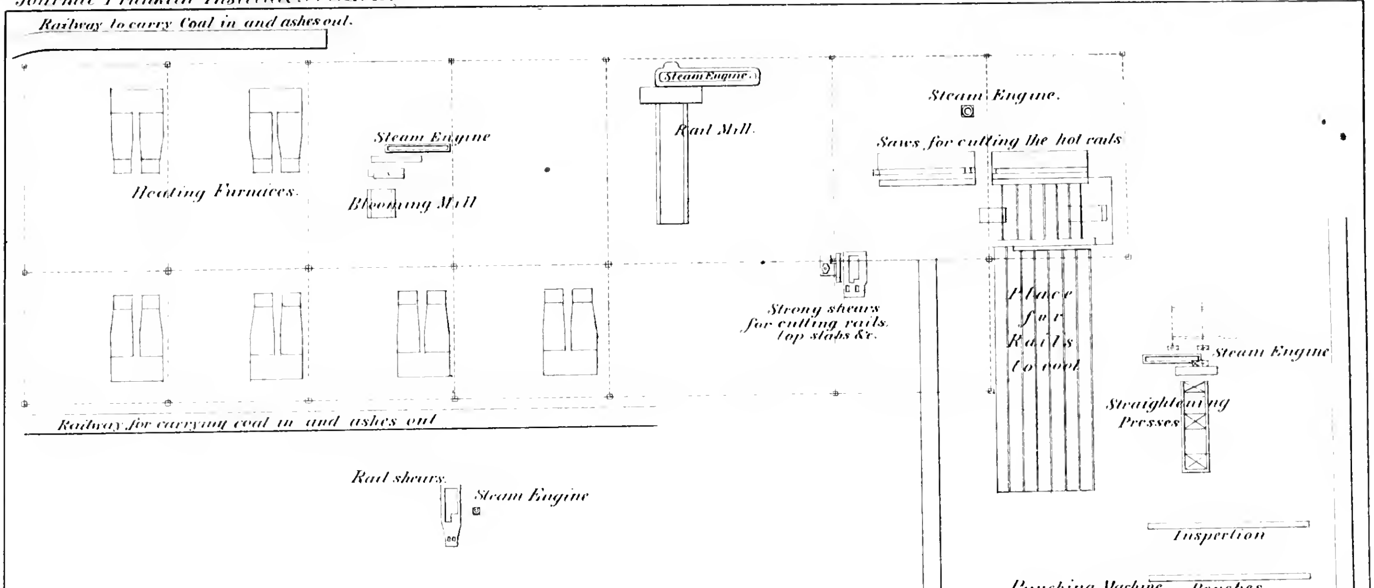
It will be readily recognized that the velocity of the fly-wheel must vary, both with the variable impulses of expansion and with the condition of application through the crank, and also with the resistance of the pumping stroke, but that its effort will be to maintain a constant angular velocity. In fact, the weight of fly-wheel must be such as will insure very small variations of this angular velocity. And in such case it is apparent that the velocity of the pumping plunger must be very nearly that known as the curve of sines, having a maximum velocity, each stroke, nearly $\frac{k}{2}$ (about $1\frac{1}{2}$) times that which is imparted by the Cornish engine, and moreover, that the starting and stopping of the water column will be at arbitrary speeds, differing from those which the simple application of an overbalancing weight, or of an elastic counterbalancing force, would have produced.

It is the losses of momentum and the frictional resistances of the water in the pumps, and passages, and pipes encountered in the transformation of a uniform circular motion into an irregular and rectilinear motion, that give the theoretic as well as the practical superiority of the Cornish engine over any possible Fly-wheel one for pumping.

I will only add, that there is yet another engine, based upon reciprocating parts, without a fly-wheel, which has the capacity of rivalling the Cornish engine in its best results, without the requirements of magnitude—almost immensity—which the Cornish engine demands.

Yours truly,

ROBERT BRIGGS.



PROPOSED PLAN OF RAIL MILL FOR

RE-ROLLING OLD RAILS WITH BESSEMER STEEL TOPS.

Scale 1/48 in = 1 Foot.

Civil and Mechanical Engineering.

THE MANUFACTURE AND WEAR OF RAILS.

By Christer Peter Sandberg, Assoc. Inst. C. E,

(Concluded from page 394.—Vol. LVI.)

As to the disposal of the rails when worn out, and as to the possibility of re-rolling old rails with advantage by companies far situated from the seat of manufacture, such as the British Colonies, the countries round the Mediterranean and the Baltic, the author thinks that, for railways near the seat of rail manufacture, the best way will be to continue to sell the old rails to the rail mills. For other countries, situated like Sweden, for instance, it becomes important to ascertain whether it would not be more advantageous to re-roll them.

The increasing traffic, the augmented speed, together with the heavier engines now found desirable, have all a tendency to more severe wear, and to render necessary more frequent renewals of the rails. These renewals are executed with more and more difficulty, as the greater number of trains limits the time at disposal, whilst the stoppage of the traffic thus occasioned often results in accidents. These considerations, together with the recurring expenses of renewals, have conduced to the employment of a more durable material for rails, as steel, which is already much used. The new rails have been manufactured either entirely of steel, or iron rails have had steel tops added to them.

In the case of steel top rails, the head has either been entirely formed of steel, or else the upper surface only has been covered with a thinner or thicker top slab, say ½ inch thick on the rail. The top slab has been joined according to the different methods used for forming the pile; so that it has been either laying flat over the wearing part of the rail-head, or it has spanned the mass of iron with its exterior edges, thus fixing the steel without reference to the weld; it has, in fact, been partly mechanically fastened to it. The mode of making the pile at several places, where old rails have been used instead of puddled bars, is shown in the plans, Figs. A, B and C. The pile used at Dowlais, for 120 tons of steel-

headed rails for the Swedish Government Railways, in September last, is shown in plan B, and the one used at Hörde, in Germany, in plan A, all for the manufacture of steel-headed rails.

The use of steel rails has much increased in England, and many railway companies are adopting them as fast as their financial circumstances will allow.

The existence of a cheap raw material, in the shape of old rails, and re-rolling them together with Bessemer steel tops, affords an opening for carrying out the manufacture of rails profitably for railway companies far removed from the seat of manufacture, even when exposed to foreign competition. If such re-rolling were carried out in Sweden, instead of selling the worn-out iron rails in England, the expense of freight to and fro would be saved, which may be reckoned at £2 per ton. The necessary cost of coal would not increase the price of rails more than about 10 per cent. per ton for re-rolling rails in Sweden.

The question is thus reduced to an inquiry, whether the increased cost of manufacture due to the smallness of the quantity to be made, would amount or exceed the remaining part of the difference of the freight, £1. 10. 0. per ton. If so, it would then be of no use to attempt the establishment of a manufacture of rails, either by the government, in connection with other workshops, for the repair and renewal of railway materials of other kinds, such as engines, axles, wheels, &c., or by private capitalists.

An estimate has been made, the particulars of which will be found in the Appendix, of the cost of manufacturing rails in Sweden, composed of Swedish Bessemer steel for the head; No. 2 iron for flange or foot; the remainder of the pile being of old iron rails. The rails are of the Vignoles Section, weighing 66 lbs. per yard.

From these calculations it is shown, 1st. That according to plan A, with the whole head of Bessemer steel, the cost per ton on an annual make of 10,000 tons, is £8. 12. 1.; and 2d. According to plan B, with the top or wearing surface of the rail only of Bessemer steel, the cost per ton, on an annual make of 6,000 tons, is £8. 6. 8.

It therefore follows that the difference between these amounts and £10, the lowest price at which such rails can be imported into Sweden from England, viz: £1. 7. 11. by plan A, and £1. 13. 4. by plan B, respectively represent the nett profit to be derived from the transfer of re-rolling operations to Sweden. In other words, providing this calculation is not too low, this represents, in the first

case, 16 per cent., and, in the second case, about 20 per cent. of the whole cost of production.

In England, the London and North-Western and the Great Western Railway Companies are re-rolling the worn-out rails at their own workshops, where the repairs of other railway materials are also executed; and, at both these places, Bessemer steel has been used for the head of the rails. The other English railway companies are selling their worn-out rails, as there is sufficient competition to prevent them being sold below the market price.

There is no railway company in France which has works for re-rolling the worn-out rails; neither is there any in Belgium, nor in Prussia, the rails being sold to the private rail mills in the respective countries, and being used, in connection with other iron, for rail manufacture.

In Austria, the State railways are carrying on the re-rolling of rails at the States' own workshop at Grätz, while the making of axles and tyres is performed at the States' workshop at Newberg.

In Russia, the rolling of the rails is executed at a workshop near St. Petersburgh, altered for the purpose by a private company. The side irons and top slabs are of English manufacture, but sometimes the top slabs are made at home from scrap-iron. English fuel is used. The Petersburgh and Moscow Railway receive in exchange for their old rails three-quarters of the weight in new rails, and pay a certain sum, according to the specification, for each weight of rails received. By the other great Russian railway, the old rails were at first sent to England; but, for some time, a private iron-work near Petersburgh, called Agareff, has carried on the re-rolling on the following conditions:—The railway company finds one ton of old rails, and receives one ton of new rails, and pays £7. 14. 0. per ton, but has then a guarantee for five years.

Having stated the case of Sweden as an example, other railways in similar circumstances, in the British Colonies, and in the countries round the Mediterranean, may be similarly dealt with. The special conditions being different in each case, make it difficult, if not impossible, to give an example that would suit every case. The principle laid down may be useful, as a guide, as to what is to be done with the rails when worn out.

In the first part of the third division of the paper, as to the best and most economical material to be employed for rails, the particular circumstances affecting Sweden are considered.

From these facts the following conclusions are drawn:—

First. That solid Bessemer steel rails, which are not likely to be manufactured at a cheaper price in Sweden than in England, or £13 per ton, are too dear to use on the Swedish railways.

Secondly. That the Swedish puddled steel rails, which cannot be manufactured for less than £12 per ton, are also too dear for the railways, even if they should last twice as long as iron rails of English make.

Thirdly. That steel top rails, at £10 per ton, are the cheapest for the Swedish railways, being cheaper than rails of Welsh iron at £8 per ton, and that it will thus become the duty of the railways' administration to procure such a steel top rail, not only for the new lines about to be constructed, but also for the maintenance of the existing lines.

In arriving at these conclusions, it must be admitted, that up to the present time, the experience of the durability of the different kinds of rails has not been sufficient to render the conclusions drawn to be thoroughly reliable.

Further: This experience of the durability of the steel top rail, and of the solid steel rail, may not agree with individual cases of failure, where, in consequence of defective welding, the steel head has come off, or where the solid steel rail has broken. At the same time it must be admitted that the process of steel-making, and of welding the steel slab to the rail is, as yet, in its infancy, so that great progress may yet be expected. The principle ought not to be condemned because of individual failures.

Assuming that, under a very heavy traffic, common iron rails 5 years, steel top rails 15 years, and solid steel rails 30 years, and that the iron rails cost £7 per ton, steel top rails £10 per ton, and solid steel rails £15 per ton, and that the old steel top and iron rails are valued at £4 per ton, and the old solid steel at £8 per ton, then with a rail section of 84 lbs. per yard, 250 tons of rails will be required for one mile of double line, and the cost of laying the rails may be estimated at £1 per ton. The following example, as to iron rails lasting 5 years, will serve to explain the way in which the subsequent Annuity Tables have been calculated.

250 tons at £7 per ton	£1750. 0. 0.
Cost of laying down	250. 0. 0.
	£2000. 0. 0.
Which sum, at the end of five years, at 5 per cent. compound interest, becomes	£2552. 0. 0.
The difference between this sum (viz: £2552) and the value of the old rails (250 tons, at £4 per ton, =£1000,) is.......	1552. 0. 0.
The annuity required to recont. this latter sum in five years, is..	£280. 0. 0.

Annuity Table No. 1.

For one English mile of double line, interest being reckoned at 5 per cent., and steel top rails being calculated to last three times, and solid steel rails six times, as long as iron rails.

Where Iron Rails last.	The Annuity would be for		
	Iron Rails.	Steel Top Rails.	Solid Steel Rails.
Years.	£	£	£
2	587	395	**325**
3	417	307	**271**
4	332	247	**245**
5	280	**218**	230
10	179	**163**	205
15	**134**	148	201
20	**130**	140	200

It may be objected that the price quoted for solid steel rails in the foregoing calculations are too high. Rails of this kind have been sold, in some places, as low as £12 per ton; but, for the very best quality, the present price is £15 per ton, and it is only from these last that the experience has been gained as to their enduring six times as long as iron rails. However, Table No. 2 has been calculated for the different kinds and periods, at the following prices, viz: iron rails at £6, steel top rails at £9, and solid steel rails at £12 per ton, crediting the old iron and steel top rails at £3 per ton, and the solid steel rails at £5 per ton.

ANNUITY TABLE No. 2.

Where Iron Rails last.	The Annuity would be for		
	Iron Rails.	Steel Top Rails.	Solid Steel Rails.
Years.	£	£	£
2	574	382	**288**
3	404	283	**233**
4	319	234	**230**
5	268	206	**174**
10	166	**149**	168
15	**133**	136	163
20	**117**	126	150

This Table shows that in all cases, except the last, solid steel rails are the cheapest.

The amount of traffic must, therefore, decide which material it is the most economical to use for the maintenance of the permanent way. For all railways, where ordinary iron rails are worn out in five years, or in a shorter time, solid steel rails are the most economical, at the price quoted in Table No. 1.

Where ordinary iron rails last over five and up to ten years, steel top rails would be the cheapest; iron rails, in these cases, being clearly proved to be the most expensive, although the cheapest where they last from fifteen to twenty years.

As these calculations are founded on the short experience gained up to the present time, in reference to the relative endurance of the different kinds of rails, a still longer trial is desirable.

The foregoing Tables refer to rails of the Vignoles Section Table No. 3 has been made up for the ordinary double-headed rail, according to the prices stated, the consideration being the same as in Table No. 2, except that the chairs have been taken into account. Allowance has been made for 140 tons of new chairs per mile, at £5 per ton, credit being given for the value of the old chairs at £2. 10. 0. per ton. It may be observed, that steel-headed rails are here estimated to last four times, and solid steel rails eight

times, as long as ordinary iron rails—that is, making allowance for the use of both faces.

ANNUITY TABLE No. 3.

Where Iron Rails last.	The Annuity would be for		
	Iron Rails.	Steel Top Rails.	Solid Steel Rails.
Years.	£	£	£
2	780	379	**296**
3	551	291	**249**
4	436	244	**228**
5	366	233	**217**
10	229	**177**	199
15	183	**166**	......
20	163	**162**	......

This Table indicates that the iron rails are in no instance the cheapest; but, on the contrary, that when iron rails last only up to 5 years, solid steel have the advantage, and where the iron rails have a longer duration, then steel-headed rails are the most economical.

It is to be hoped that railway companies having a heavy traffic will give different sorts of rails great attention, and submit them to trials on a large scale; and, on the other hand, that the steel-works will try their utmost to manufacture solid steel, as well as steel-headed rails, of the best sort, for the purpose, so that this important question may soon be decided.

Before concluding, another fact must be taken into consideration, viz: the safety of the three different materials in regard to high speeds, severe climate, &c.

This seems, of late, to have engaged the attention of the railway world, and has been discussed not only in England, but on the Continent. The Swedish Government having undertaken the construction of railways in that country, appointed a committee, composed of many eminent men, to consider it. This committee found it necessary to make experiments with different materials from England as well as Sweden; and, after five years' consideration and study, the report has just been published by Professor Styffe, the

Director of the Government School of Mines at Stockholm. From this report, it appears that the tenacity and elongation of different materials are influenced by the amount of carbon.

The Tenacity Influenced by Carbon.

DESCRIPTION OF MATERIALS.	Carbon. Per Cent.	Elongation. Per Cent.
Swedish Bessemer Steel, Uchartin's Steel....	1·85 to 1·0	0·3 to 0·9
For Cast Steel..	0·69 " 0·61	1·2 " 2·1
Bessemer Steel or Iron..........	0·42 " 0·33	1·9 " 4·
	Spec. Grav.	
Iron from lake ores, rich in phosphorus......	"	0·8 " 3·4
Iron from Dudley, rich in flag & phosphorus.	7·5	2·5 " 4·2
Iron from Middlesboro or Tees..................	7·65	3·4 " 5·9
Puddled Iron, from Sweden and Lon. Man..	7·77 to 7·80	6·1 " 9·5
Swedish Iron, made in refining furnace	7·84	7·3 " 7·8

The Absolute Strength Influenced by Carbon.

DESCRIPTION.	Carbon. Per Cent.	Weight in lbs. per square inch when broken.
Swedish Charcoal Puddled Iron................	0·8	113·381
" " " "	0·7	84·265
" Bessemer Steel	0·8	90·921
" " "	0·55	86·941
" " "	0·50	71·099
" " Iron...............................	0·20	48·102
" Puddled "	0·70	83·441
" " "	0·70	83·716
" from another work..........................	0·6	73·492
" " "	0·6	82·344
" " "	0·5	78·432
" " "	0·7	86·049

These Tables show that the hardest material has the greatest absolute strength, both before and after permanent set has taken place, but it has the least ductility; on the other hand, a softer material shows the greatest tenacity or elongation; the Bessemer material giving the same result as that prepared from the same pig iron, by puddling, refining, or the cast steel process.

In the diagram illustrating these results, the percentage of carbon and of phosphorus is stated in nearly all cases. The limit for the amount of carbon seems to be for the Bessemer material 1·2 to 1·5 per cent. With a larger amount, the absolute strength, as well as the tenacity, has been found to decrease. When the amount of carbon does not exceed 0·4 per cent., and the material is not worked at too low a heat, the elongation seems to be 16 per cent., or the same as for puddled iron from the same pig iron; and as such Bessemer material is not only much stronger, but also more solid or homogeneous than the puddled material, it deserves a decided preference for all railway purposes.

The few cases of failures of rails by breaking, may be accounted for as the result of too hard a material, not perfectly manufactured, having been made at the earlier period of the introduction of the process. The experience which has now been gained should certainly prevent any recurrence of this.

Iron and steel, when tried for tensile strength under the influence of extreme temperatures, such as boiling water, and at the freezing-point of mercury, has led to the discovery, (contrary to the general belief,) that the tensile and absolute strength is greater during cold than during ordinary temperature—that is, iron or steel is stronger in winter than in summer.

The reason why more breakages occur in winter than in summer is asserted to be due to the external cold affecting the elasticity of the supports (sleepers); and that elasticity in any way given to the rolling stock also favorably affects the resistance of the rails.

However, if the supports have the same elasticity in summer as in winter, as, for instance, would be the case with granite rock, then, Professor Styffe asserts, that the same rails, either of iron or of steel, can resist a heavier blow from a falling ball, at the temperature of extreme cold than on a hot summer's day. Although the experiments have been conducted with the utmost care and skill that science and money can afford, it seems desirable that this theory

should be proved on a larger scale than Professor Styffe has had an opportunity of doing, before it can be relied on.

At a meeting of engineers, at Stockholm, in March, 1867, it was decided that Bessemer steel rails, made from charcoal pig iron, might, without risk, be used 10 per cent. lighter than the English iron rails; and in Austria, this has already been practiced with success by the Engineer-in-Chief, Wöhler.

It must, however, be observed, that the raw material used in both cases, is charcoal pig iron of a superior quality, as compared with that used in England for making Bessemer rails, which may be seen from the following analyses made by two eminent chemists:—

Bessemer Swedish Pig Iron. Fagersta Works. Analyzed by Kohlberg.	Per Cent.	English Bessemer Pig Iron. Workington, Cumberland. Analyzed by John Percy.	Per Cent.
Graphite	2·733	Carbon	2·993
Combined Carbon	2·138	Silicon	3·080
Silicon	0·611	Manganese	0·079
Manganese	2·926	Sulphur	0·021
Sulphur	0·015	Phosphorus	0·021
Phosphorus	0·026		

These analyses show that the great difference between the two is the excess of silicon in the English, and of manganese in the Swedish pig iron, thus explaining why the one gives a better result than the other, although worked entirely without the addition of spiegeleisen.

If there be only 0·6 per cent. of carbon in the solid steel, and 0·6 per cent. in the steel head, the safety ought to be the same for all the three kinds, and this would not influence the calculations as to which is the best and most economical material for rails.

Having watched the development of the Bessemer process in England, as well as on the Continent, it seems to the author that, by that process, a good and pure raw material has the same advantage over an inferior one as in all other processes, and that a superior product cannot be obtained from an inferior raw material by that process any more than by others.

In having mentioned Swedish material as an example, it must not be supposed that it is wished to advocate the use of Swedish iron in this country, but simply to draw attention to the better material, as equally good charcoal iron can be supplied from Canada and India, both English Colonies. It may also be remarked that the author's endeavor has been to arrive at the truth, irrespective of prejudice, and that he has no wish to be deemed an advocate for one kind of rail more than any other.

ST. LOUIS AND ILLINOIS BRIDGE.

We have received, and publish with pleasure, the following letter, a copy of which has been sent also to Mr. Colburn, and will doubtless be published by him in his journal.

ZERAH COLBURN, ESQ.,
Editor "Engineering."

Dear Sir:—In the issue of your valuable *Journal* of September 25th, we find an article on the Mississippi Bridge, at St. Louis, designed by Capt. Jas. B. Eads, which, while favorable to the general plan of construction, discredits the correctness of some of the results of our investigations. We would have replied promptly, had we not preferred to await an expression of Capt. Eads' views on the subject. Capt. Eads has now, in a letter dated at Rouen, Italy, stated his opinions, which in the main, coincide with our own, and has desired us to answer the objections raised by you.

The first and principal objection against the correctness of our results you base on the supposition, that "an arched rib, *per se*, is neither more nor less than a long column."

To support this assumption, you refer to the fact, that in a long column of uniform cross section, subject to two equal and opposite end forces, acting at the centre of gravity of the section, in some cases positive tension is produced by compressive forces, in consequence of variation in the elasticity of the material, though mathematically, the unit strain should be uniform throughout the entire column.

The variations in the elasticity of the material in any cross section are either symmetrically disposed in regard to its centre of gravity, or unsymmetrically. Under the first supposition, they would have no tendency to produce a bending of the column, and

their sole effect would be a distribution of the strain not uniform but proportional to the moduli of elasticity of the elements of the section. In a material of good quality, these moduli cannot vary greatly, and the effect of such symmetrical variations may therefore be neglected in the calculations.

The case is quite different, however, when the modulus of elasticity is smaller on one side of a section than on the opposite side. Then, even if the resultant of forces passes through its centre of gravity, the fibres on the softer (more compressible) side must undergo greater compression, before they offer the same resistance, as the fibres on the harder side, and this causes a deflection of the centre line of the column.

In the succeeding cross sections, the resultant no longer passes through the centre of gravity, and the unit strain must be greater on the softer than on the harder side. The tendency to bend, resulting from the unequal elasticity, is therefore materially increased by the moment of the resultant about the centre of gravity.

It is easily perceived, that from this rapidly increasing tendency to bend, in long, thin columns, considerable deflection may arise, which might finally carry the resultant so far outside the centre of gravity of the section, that the strains produced by the moments of flexure would exceed those due to the direct load, and would produce tension on one side.

The danger in the use of such long, thin columns consequently results from the fact, that a moment of flexure may occur, while *none* can be considered in the calculations.

In the arched ribs of our bridge, the case is quite different.

If you examine our calculations, you will find that moments of flexure are introduced. We consider the rib not as a member subject to direct compression alone, but as a beam subject to the action of direct and transverse forces. The sectional area of the ribs is consequently made much larger than would be necessary to resist the greatest possible direct compression. The latter could be met by an almost uniform cross section of—

$$1{\cdot}8 \times \frac{515^2}{8 \times 51{\cdot}5} \times \frac{1}{12{\cdot}5} = 93^{*} \text{ square inches, when}$$

1·8 = load in tons per lineal foot of rib.
515 = span in feet.
51·5 = rise of the arch in feet.
12·5 = allowable strains in tons per square inch.

* See Plate I., Diagram 2, of Capt. Eads' Report.

But, without ever considering the effects of temperature, our calculations demand sectional areas, varying for different points from 102 to 180 square inches.

The opposite strains (tensile on one side and compressive on the other), induced by the moment of flexure, and which added to the effects of direct compression, cause an unequal distribution of the strains, are proportional to the distance of the resultant from the centre of gravity of the section. Now it is certain, that the unequal distribution of the load will cause a much greater deviation of the resultant from the centre line of the arch, than any variations in the elasticity of the material could ever produce, especially when, as in our bridge, the rib does not consist of one single piece, but is composed of a great number of smaller pieces, thus admitting of a much greater uniformity of material. The strains arising from variations in the elasticity of the material cannot, therefore, be more than a small fraction of those considered in our calculations.

But, even when considered as a curved column of the proportions of length to breadth = $\frac{1}{64}$th, and loaded to the full bearing capacity of its cross section, this rib would be fully strong enough.

It is stated by some of the best authorities on this subject, that solid wrought iron columns, whose ends are fixed in position (as are the arched ribs of the Mississippi Bridge), may be calculated as resisting direct compression only, as long as the proportion of breadth to length does not exceed $\frac{1}{48}$ in a column of circular section, and $\frac{1}{56}$ in a column of rectangular section. As our ribs are to be constructed of a much stronger and harder material than wrought iron, and as our cross section (in which all the material concentrated at both extremities, resists deflection with its full force), insures much greater stiffness than a rectangular one would produce, it becomes evident that even under this supposition, we kept within the limits of safety. Our margin of safety is considerably increased by assuming the proportions given by you, viz., $\frac{1}{45}$. As to the remark, that you are "at a loss to guess what reasons induced us to assume a depth of eight feet in our rib," we would refer to page 4, of Capt. Eads' Report, where you will find, that calculations were made for ribs of various depths, and their results indicated eight feet as the most economical. An increase of the depth of the rib admits of a reduction of the amount of material required to resist an unequal distribution of the load, while at the same time it requires an increase of material to resist the effects of changes

of temperature. The depth of eight feet gave the smallest amount of material to resist these combined factors of strain.

In regard to the stiffness of our bridge, which you seem to doubt, we would most respectfully refer you to page 38 of the Appendix to Capt. Eads' Report, where the vertical deflection of the rib under the most disadvantageous distribution of the load is calculated as not exceeding 2·69 inches, and the greatest horizontal deflection 0·8 inches.

Your objection in regard to the omission of spandril bracing, becomes nugatory if, as we think we have shown, the rib possesses sufficient stiffness in itself.

Very respectfully, your obedient servant,

(Signed), HENRY FLAD,
CHAS. PFEIFER.

THE STREET TUNNEL AT CHICAGO AND ITS MACHINERY.

BY PROF. S. W. ROBINSON, C. E.

AT the time of my visit to the Chicago Washington Street Tunnel, on the 13th inst., the work of tunneling under the river was considerably more than half completed. The approach and arched way on one side are entirely finished as far as to the centre of the river, and work has begun briskly on the opposite bank.

The bed of the river consists of soft, tough clay. It is therefore impracticable, if not impossible, to drive the tunnel without unroofing it, especially under the river's bed. And as it is necessary to continue the work without interrupting the river navigation, and still uncover the roof, it became necessary to establish a coffer-dam that should extend to only about half of the river's breadth at one time. By means of such a dam, the tunnel has been completed up to the centre of the river, and covered again. The dam was then transferred to the opposite shore, shifting at the same time the passage for boats.

The second dam was completed on the 13th inst., and the water partly withdrawn with the pumps still working. Two pumps were in operation; a rotary and a vacuum pump. The latter is of new design, and deserves more particular mention. It will directly be more fully described. The work of uncovering the tunnel bed has begun on this side just beyond the dam, or river bank, and proceeded to a depth of fifty feet, with about the same length and

breadth. When the coffer-dam is emptied, the excavation will be extended into it, and the finished part of the arching brought forward beyond the limits of the river and covered. The dam can then be removed from the river entirely, and the tunnel continued to its second approach.

Two power derricks are in use for hoisting and shifting the excavated material, only a part of which is required to be taken away, viz.: a volume equal to that of the tunnel, the approaches and the masonry. The balance is to be transferred from its native bed to the top of the tunnel. In doing this, the derricks command about a cubic yard at a lift, which is raised at the rate of a foot per second, taking and depositing it about forty-five feet from the centre mast.

The clay is so soft that the sides of the excavation must be supported to protect the foundations of adjacent buildings. This is effected by means of cross timbers pressing against plank walls. The clay is handled with hay forks, it being previously cut into lumps with sharp spades. It was noticeable that the workmen were very careful to entirely free every lump, that it should not be found tied down by the tenacity of a small unsevered fragment. On the whole, the work appeared to be progressing very favorably, with a good prospect of a speedy and successful termination.

The vacuum pump spoken of above, is simple and, it is said, very efficient. It consists of a cylindrical chamber of wood, strongly hooped, being about three and a half feet in diameter, and about five feet in height; having suction and discharge pipes with valves properly arranged, and a steam pipe leading from a boiler; together with a small injector vessel supplied with water. Steam of the desired pressure is forced into the chamber by the steam pipe, when the admission is arrested by a three-way valve, a branch pipe leading therefrom, terminated by a check valve, allows the escape of steam from the chamber until the pressure of the atmosphere is reached. As soon as this occurs, a valve drops in the vessel upon the top of the chamber, introducing a spray jet, by which the remaining stream is condensed, thus forming the vacuum. The valve of the suction pipe is then raised, and water rushes in from below and fills the chamber. This done, steam is again forced into the same chamber in contact with the water, which promptly retires before it through the discharge pipe. The small injector vessel is filled with water from the discharge pipe. When the water

is driven out, the three-way valve checks the steam, and the vacuum is again formed, and the above described operations repeated, which may be so continued indefinitely. The three-way valve was operated by a person stationed for that purpose. The water was raised about twelve feet, to be discharged over the dam. By a similar machine, it is said water has been raised in a jet to a height of over 100 feet.

A question is naturally raised as to whether this is an economical method of using steam. Let us consider this subject briefly. It was ascertained by actual observation with thermometers, that the water discharged over the dam by this pump, was increased in temperature two degrees. By calculation it is determined that the condensation of the steam remaining in the chamber at atmospheric pressure, is sufficient to raise the temperature of the chamber full of water about two-thirds of a degree. There is then imparted one and one-third degrees of heat to the water by reason of the admission of steam to contact with it, and with the wet interior surface of the chamber. The prejudicial result effected by operating this contrivance, is then represented by the mechanical effect equivalent to heating all the water discharged one and one-third degrees. It was determined by Joule and Mayer, independently, that to raise the temperature of one pound of water one degree, requires a mechanical effect equal to raising 772 pounds one foot high. The volume discharged per minute, at the rate of three strokes, is 144·3 cubic feet, or 9018·7 pounds. To raise this twelve feet per minute, requires three and three-tenths horse-power. But to heat it one and one-third degrees, requires per minute, a mechanical effect equal to $9018{\cdot}7 \times 772 \times 1\frac{1}{3} = 9280244\cdot$ ft. pounds $= 281$ horse-power, But the steam was all taken from a fifteen or twenty horse boiler. What, then, is the mechanical equivalent in this case?

But let us take another view of the case. To heat a given quantity of water one and one-third degrees, takes twice as much steam as to heat it two-thirds of a degree. Then if 144·3 cubic feet of steam, at atmospheric pressure, will heat the same volume of water, at the ordinary temperature, two-thirds of a degree twice the quantity of steam at atmospheric pressure will raise the temperature of 144·3 cubic feet of water one and one-third degrees. If the last named quantity of steam, which is the amount lost by incidental condensation, be used each minute in a steam cylinder at sixty pounds apparent pressure, it would perform the work of two and

a half horses. Taking this as the prejudicial work of the vacuum pump, and 3·3 horse-power, which was shown above to be the theoretical work required to raise the water, we find that the work lost is 0·76 of the useful effect; and 0·43 of the total effect; which is a large per centage for even ordinary pumps. If this is true, it appears from the results of the experiments cited, that as a device for raising water, the vacuum pump is not an economical machine, except where convenience demands its use, or where its temporary employment justifies the application in preference to more costly machinery.

One of the most essential pre-requisites for economical results with this device, is, that the surfaces with which the steam and water have alternate contact, must be non-conductors of heat. Indeed, the measure of success depends upon the degree to which this principle attains. One might at first naturally suppose that the cold water surface would contribute to rapid condensation of steam. But it is very well known in the science of physics, that water is one of the poorest conductors of heat; which becomes particularly apparent in the downward direction where conviction fails to act. A remarkable statement, substantiating this fact, on a large scale, appeared in the *Scientific American*, early last spring, in which it was confidently asserted that one of two boilers of moderate size, established in separate arches, connected by a steam pipe having no separating valve, had frequently been fired while the other was not, although still containing the ordinary supply of water; and that the water in the unfired boiler had not changed materially in temperature after a considerable interval of time. From this, together with the facts relating to the vacuum pump, it appears that water is a poorer conductor of heat than the wet wood of the interior of the chamber, as the amount of condensation incident upon the ejection of water is very considerable. If so poor a conductor as wood is barely suitable for the interior of the chamber, iron could not possibly answer, notwithstanding the want of its strength to guard against explosion; although iron cylinders lined with wood or other non-conducting material, might serve the purpose well, some good non-conducting material having little inclination to retain water, as wet would form the best lining. That so good a conductor as iron cannot form the interior surface, is strikingly shown by the performance of certain surface condensers. In Hamilton, Ontario, at the machine works of the Northey Brothers, a twelve

horse engine exhausts into a cylindrical wrought iron chamber about two and a half feet in diameter, and thirty feet high. It is of boiler plate, about five-sixteenths of an inch thick. A stream of water delivered upon the top flows down over the whole exterior surface. This chamber maintains nearly a perfect vacuum.

S. W. Robinson.

University of Michigan.

THE ECONOMICAL CONSTRUCTION OF BEAM TRUSSES.

By G. S. Morison, C. E.

(Concluded from page 399.—Vol. LVI.)

Case of Four Continuous Spans.

The two central spans are supposed equal, and each one-quarter larger than the end spans, and as such a bridge is perfectly symmetrical, only the first two spans need be examined. The truss is supposed to be a Warren girder, single system, arranged for a through bridge, the floor being supported on the bottom chord at he end of each panel, and hung by a vertical tie from the upper chord at the panel centres. The depth of truss is taken at thirty feet, and the length of panel the same; the number of panels in the end spans sixteen, and in the central spans twenty, making the length of these spans respectively, 240 and 300 feet. The dead load is assumed to be 1,000 pounds per foot, and the moving load 1,500; making for the bridge of two trusses a total load of 5,000 pounds per foot. This form of truss is selected on account of its simplicity, an equal amount of weight being thrown upon the truss at the intersection of each brace with either chord.

Representing the chord strains at the five points of support by s_1 s_2 s_3 s_4 and s_5, the lengths of the four spans by l_1 l_2 l_3 and l_4, and their respective loads per foot by w_1 w_2 w_3 and w_4, we have—

$$l_2 = l_3 = 1{\cdot}25 l_1 = 1{\cdot}25 l_4 = 1{\cdot}25 l, \quad s_1 = o \quad s_5 = o$$

and equation (6) gives—

$$18\, s_2 + 5\, s_3 + \frac{l^2}{h} w_1 + 1{\cdot}953125 \frac{l^2}{h} w_2 = o$$

$$5\, s_2 + 20\, s_3 + 5 s_4 + 1{\cdot}953125 \frac{l^2}{h} w_2 + 1{\cdot}953125 \frac{l^2}{h} w_3 = o$$

$$5\, s_3 + 18\, s_4 + 1{\cdot}953125 \frac{l_2}{h} w_3 + \frac{l^2}{h} w_4 = o$$

solving these three equations—

$$s_2 = \frac{l^2}{h}\left(-\cdot 06004 w_1 - \cdot 085755 w_2 + \cdot 022751 w_3 - \cdot 00448 w_4\right)$$

$$s_3 = \frac{l^2}{h}\left(\cdot 01613 w_1 - \cdot 081905 w_2 - \cdot 081905 w_3 + \cdot 01613 w_4\right)$$

With reference to the first span, two cases only need be considered; when s_2 is most intense, first, second, and fourth spans loaded ( ) and when the strain at the centre is greatest first and third spans loaded ( ). With reference to the second span, three cases must be examined, when s_2 is most intense)), when s_3 is most intense, second and third spans loaded ( ) and when the strain at the centre of the span is greatest, second and fourth spans loaded ( ). These cases with their complements form three groups, two of which will be applied to the first span, as follows—

Spans 1 and 2.

$w_1 = w_2 = w_4 \quad w_3 = w_0$

Spans 1 and 2.

$w_1 = w_3 \quad w_2 = w_4 = w_0$

Span 2.

$w_1 = w_4 \quad w_2 = w_3 = w_0$

For these three groups, the equations expressing s_2 and s_3 become

$w_1 = w_2 = w_4 \quad s_2 = \frac{l^2}{h}(-\cdot 15028 w_1 + \cdot 02275 w_0) \quad s_3 = \frac{l^2}{h}(-\cdot 04965 w_1 - \cdot 08191 w_0)$

$w_1 = w_3 \quad s_2 = \frac{l^2}{h}(-\cdot 03729 w_1 - \cdot 09024 w_0) \quad s_3 = \frac{l^2}{h}(-\cdot 06578 w_1 - \cdot 06578 w_0)$

$w_1 = w_4 \quad s_2 = \frac{l^2}{h}(-\cdot 06452 w_1 - \cdot 06300 w_0) \quad s_3 = \frac{l^2}{h}(+\cdot 03226 w_1 - \cdot 16381 w_0)$

The symbol w_0 is used to denote the weight per foot of those spans differently loaded from the first span.

Substituting the numerical values of l, h, w_2 and w_0, we have for each of the six cases under consideration—

	$s_2 = -677650$ lbs.	$s_3 = -395570$ lbs.
	$s_2 = -179330$ "	$s_3 = -488190$ "
	$s_2 = -352240$ "	$s_3 = -442030$ "
	$s_2 = -504740$ "	$s_3 = -442030$ "
	$s_2 = -430690$ "	$s_3 = -159690$ "
	$s_2 = -426290$ "	$s_3 = -724370$ "

It will be seen that the sum of the values of s_2 or of s_3 for any

pair of complementary cases is a constant; the same is true of the chord strains at any point in the bridge.

Having determined the strains over the points of support, it remains to draw the general curves of strain. These are given on Plate 4. The parabolas corresponding to each pair of complementary cases are drawn in similar dotted lines, thus showing at once their connection, and making evident their complementary character; the curves denoting the strains in unloaded spans are easily distinguished by their flatness. The heavy line, A D E F C H I K L, shows the maximum strain at every point without regard to size (B F being equal to B M and C L to C N), except between D and E, G and H, and I and K, where a generous allowance is made for irregularities at and near the points of reversal, this allowance being such as to make the section of the proportioned chord between these points about one-half the maximum section. For the sake of comparison, the curves of strain in the same spans when isolated are given also; these curves are the parabolas A O B and B Q C, the maximum strain in the first span being 600,000 pounds, and in the second, 937,500 pounds. The figures on the truss below are taken by measurement from the diagram, and express in units of 10,000 pounds the chord strains in each panel. The curves for the unloaded spans all fall within the limiting line, though about the points of reversal they indicate that the maximum strain occurs when the span is unloaded. Case third, shows that at times the chord strains may be negative through the whole length of an intermediate span. This might occur even in an end span, if the end spans were very much shorter than the intermediate, but as the effect would be to lift the end of the bridge from its abutment, so great an inequality should be carefully avoided.

The average maximum chord strain in the first span (including the surplus about the points of contrary flexure), is 348,750 pounds, and in the second span, 403,500 pounds. In an isolated 240 feet span of the same depth, and carrying its same weight, it would have been 400,000 pounds, and in a similar 300 feet span, 625,000; the reduction due to continuity being about 13 per cent. in the former span, and over 35 per cent. in the latter span, and over 27 per cent. in the whole bridge; and this reduction is on the supposition that the dead loads are equal in each case, whereas they would really be much greater in the isolated spans.

In the first span, the shearing strain will always be less at A and

greater at B than if the span were isolated; a change which continues through the span, and throws the mean vanishing point back towards A. The greatest strain at A occurs in case 3, and is equal to—

$$\frac{(w + w^1)\,l}{2} + \frac{s_2\,h}{l} = 255970 \text{ pounds.}$$

and the most intense strain at B, in case 1, it being—

$$-\frac{(w + w^1)\,l}{2} - \frac{s_2\,h}{l} = -384{,}706 \text{ pounds.}$$

In the second span the maximum strain at B occurs in case 1, being the 403,208 pounds, and the maximum at C, in case 6, when it is 404,808 pounds. Hence it appears that in the first span for a part of its length, the web may be made lighter than in an isolated span, but for the greater part of this span it would be heavier, while through the whole length of the second span it would be a little heavier than in a disconnected span; the counterbraces, if the web does not counterbrace itself, must also be carried farther on each side of the centre. The increase will not exceed 14 per cent. of the weight of an isolated web in the end span, nor 16 per cent. in the intermediate, an increase which, as the weight of the web is always much less than that of the chords,* may be disregarded by the side of the great saving effected in the chords.

In spite of the great economy of continuous spans, their adoption has not met with universal approval. The occasional failure of bridges built in this way, has caused an unfortunate prejudice against them, but these failures have occurred in bridges in which the spans, though made continuous, were built on the same principles as if isolated, for the most part in wooden structures, and are to be attributed solely to the ignorance of the engineers who built such structures. The failure has usually been in the top chord, which being packed only to resist compression, has been torn apart over the piers where it is heavily strained in tension. On having the upper chords cut, these bridges have done well, as they are then subjected to strains only of that character which they are adapted to bear. Such failures, occurring exactly where theory would have predicted them, rather prove the correctness of the reasoning by which continuous spans are proved to be economical, than furnish any argument against their use.

* Theoretically, the weight of the web in an isolated span, does not become equal to that of the chords, until the depth of truss is made about one-third the length.

In an isolated span the top chord is strained through its whole length in compression, and the bottom chord in tension; each chord may be constructed of that form and material best adapted to its particular strain, and effective connections will be made by cheap and simple details. In a continuous girder, on the contrary, this unity of action is lost, and each chord is liable to be strained for a considerable part of its length, alternately in tension and compression. In structures entirely of wood, this would be a source of little difficulty, as a wooded chord properly packed to resist tension, acts equally well in compression; in composite structures, where wood is only used to resist compression, continuous spans become impracticable; while in structures entirely of iron, cast iron must be excluded, and both chords constructed of wrought iron alone. The experience of engineers, however, has not been favorable to the use of cast iron, even in isolated spans, and it is not unlikely that the general adoption of a form of bridge, which would compel its abandonment, would be an advance in the direction of safety. If the details of a chord, intended to act only in one way, are much cheaper than those of a chord adapted to both kinds of strain, this is a matter of practice rather than of necessity, and there is no good reason why a simple detail may not be contrived, which shall prove an effective connection both against tension and compression. The straining of the same iron in opposite ways, has been supposed to act injuriously, but recent experiments indicate that it is otherwise, as might be expected, from the fact that opposite strains tend to retard the permanent set, which is sure to weaken eventually any material subjected continually to the same kind of a strain.

The calculation of strain in a continuous girder is longer and more difficult than in an isolated span, and the results obtained are not so strictly accurate. The strains over the piers of a continued truss are deduced from the curvature of that truss when strained by the action of a load, this curvature being assumed to be everywhere proportional to the chord strains, an assumption which is strictly true, if the chords are everywhere of uniform section, but which leads to a slight error when their section is made variable. This error is at most very slight, and of a kind easily guarded against. The effect of increasing the sections of the chords at the ends of the spans, where the strain is most intense, is to flatten the curvature at the ends, thus throwing the points of reversal towards the centre

of the span, and consequently increasing the strains over the piers, and diminishing that at the centre of the span. This variation will be effectively counteracted, but putting a slight excess of material in the chords of the end panels, but it is so slight that the best European engineers have considered that it may be safely neglected. In the multitude of uncertainties about the action of a moving load, even in isolated spans, the peculiar effects of which are the result not only of its character as a variable partial load, but are also due to action of motion itself, the vibrations caused by the rapid succession of shocks, and many other irregularities which can be guarded against only by the adoption of a large factor of safety, any slight variation like that alluded to above, especially if its general character be understood, may readily be set aside. Still, though the results we now have may be abundantly accurate for practice, there would be no small satisfaction in having them made strictly correct, and, in the language of an eminent French engineer, though "trusses constructed in this way act favorably, and resist well all the tests to which they are subjected, a fact now proved by numerous examples * * * there is no doubt that if we could succeed in so altering the principles of the method as to render it more rigorous without making the use of it much too long and difficult, constructors would be glad to accept such a change which would mark a real advance in science."*

First among the merits of continuous spans, is their great economy, which can hardly be overrated, as continuity reduces the weight of an intermediate span nearly one-third, and makes it possible to contract spans of three hundred feet, at a cost per foot but little in excess of that of a two hundred foot span, thus saving the cost of a foundation in six hundred feet; but there are other advantages of too great weight to be entirely overlooked. In an isolated span, the truss is heaviest at the centre, where weight acts at the greatest disadvantage, but in a continuous girder, the weight of material both in chords and web is near the piers, where it imparts but little strain to the truss. When the spans of a bridge are disconnected, the weight of each span must rest on or near the edge of the piers, and the action of a heavy passing load will be to throw the additional weight first upon one side of the pier, and then on the other; in a continuous girder, the bridge seat may be placed directly over the centre line of the pier, so that any additional weight will be thrown on

* Bresse. Avant Propos. Partie IIIme. Mecanique Applique.

the axis of the pier, and the tendency to rock reduced to a minimum. The deflection of a span is less if it is connected with the adjoining spans than if standing alone; this diminution being due both to the decrease of strains and to the reversals of curvature, the same amount of curvature causing a greater deflection if the curve be always in one direction, than if it be broken and reversed.

As regards facility of erection, the advantage usually lies with disconnected spans; but in positions where it would have been difficult to erect suitable false works, or when it has been considered desirable to have the construction of the superstructure go on simultaneously with the building of the piers, foreign engineers have often built continuous trusses on the land at the end of the bridge-line, and on the completion of the masonry, moved the whole structure forward into position, a process which would have been impossible with disconnected spans. In a bridge with riveted chords, it is no small advantage to be able to put the whole together on the land where the riveters will have abundant room for their forges and good standing room; but if it be considered preferable to erect the structure in place, there is no inherent reason why the parts of a continuous truss should not be as completely finished in the shop and as rapidly put together in place, as those of an isolated span.

BELTING FACTS AND FIGURES NO. II.

By J. H. Cooper.

(Continued from page 388.—Vol. LVI.)

Joining the Ends of Belts.

"Shoe-pegs are successfully used here for joining belts to stand the effects of water and oil."—T. G., Providence, R. I., in *Sci. Amer.*, January, 1854, p. 147.

"After many plans of waxed-ends, laces, rivets and bolts, with and without plates, have been tried in joining belts, the best plan, in my opinion, for a permanent joint, is to scarf the ends as usual, then glue and bind the ends together with hand-screws, until the glue is set, then apply as many shoemaker's pegs as are necessary, dipping each into glue before driving it in. The pegs are then pared smooth on both sides, and the joint made of equal thickness with the rest of the belt.

"If not exposed to water, I will warrant this joint to last as long

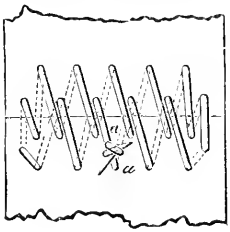
as any other part of the belt."—Machinist, in *Sci. Amer.*, March, 1854, p. 217.

"If pegs are used, they should be inserted on the side next the pulley."

The usual way of joining the ends of a belt—that is, by means of the leather thong—is the best after all, because it is the most convenient; the thong being an article more readily obtained and applied than any other of the numerous and ingenious means devised for securing the ends of a belt.

In the use of thongs, it is the practice of some engineers to cross them in lacing on both sides of a belt; with others, to cross them on the outside only, laying the double strands evenly on each other in the line of motion and on the pulley side of the belt, which experience proves to be the better way.

We present, below, a plan, altered from the original, in which the two ends, *a a*, of the thong are tied in the middle of the belt.



"I have had experience for upwards of twenty years in such matters, and do not cross the lacing at all. I make two rows of holes, as shown in the engraving.

"By this plan, I find a lace will last twice as long as it will when crossed."—W. A., in *Sci. Amer.*, January, 1866, p. 52.

To measure the length of a belt in coil, use the following formula.

$$L = (D + d)\, n \cdot 1309$$

In which L = length of belt in feet.
D = mean diameter of roll in inches.
d = " " eye "
n = number of turns.

W. S. A., in *Sci. Amer.*, December, 1866, p. 423.

Effect of disproportion of connected machinery.

Sometimes a belt works badly from causes outside of its own motion and proportions.

We have a case in practice which will forcibly illustrate this. A 46-inch pulley, on the "line" shaft, drives a 5-foot pulley on a 4-inch shaft, at the rate of 73 revolutions per minute, by a 12-inch open belt. This shaft is 7 feet 8 inches below, and 2 feet aside of the "line" shaft, and carries an 8-foot fly-wheel of 3,750 lbs. weight on its middle, and a crank with a double pin on its overhanging end, which latter is connected with and drives two marble saw-frames,

one very heavy, the other of medium size. The belt runs slack and free, and has not been touched at the lacings during six months of very steady and satisfactory running.

Before the 8-foot fly-wheel was put on, a 6-foot fly-wheel, of about 1,450 lbs. was used, which a long, troublesome experience proved altogether inefficient. The belt had to be run very tightly; it tore frequently at the lacings—even when the laced ends were doubled to make the stronger joining—and at all times while running, the lack of momentum of the wheel caused unsteadiness of motion in the whole system of gearing in the mill.

Driving Power of Belts.

"As regards the width of the belt, this will be found ample with respect to friction, if we calculate the cross-section of the same for the strain to be transmitted, in which case one-eighth of an inch square is allowed for every 5 lbs. strain."—C. D. Abel, in *Weale's Series.*

"The working strain of a 9-inch belt, on 4 feet pulleys, under the effects of a tightener, in use nearly three seasons, at a velocity of 1,571 feet per minute, without ever failing, was found to be 28·3 lbs. per inch of width. Experiments proved that an increase of 25 per cent. of the above load endangered the splicings and safety of the belt."—J. A R., in *Sci. Amer.*, July, 1865, p. 68.

"Morin concludes that we may, without any risk, and with the certainty that they will run a long time, make them support tensions of 355 lbs. per square inch of section."—*Frank. Inst. Jour.*, July, 1844, p. 27.

"Good belting of, say, $\frac{3}{16}$ inch thick, should sustain a tensional strain of 50 lbs. per inch of width, and without serious wear, for a long time."—*Appleton's Mech. Mag.*

Haswell, in his Engineer's Pocket-Book for 1867, says: "A leather belt will safely and continuously resist a strain of 350 lbs. per square inch of section."

We are indebted to Mr. R. H. Thurston, U. S. N., for the following:—

$$W = \frac{7000 \times HP}{SV}$$

In which W = width of belt in inches.
HP = indicated horse-power transmitted.
S = portion of circumference of smaller pulley covered by belt, in feet.
V = velocity of belt, in feet, per minute.

Mr. Thurston considers 100 lbs. per inch of width on ordinary

belting of, say, $\frac{3}{16}$ inch thick, a fair working load. Then calling t = tension, and inserting same in the formula above, we have:—

$$W = \frac{700{,}000 \text{ H P}}{S\,V\,t}$$

From a treatise on machine belting, published by Hoyt Brothers, of New York, we extract the following:—

"The Table inserted below, gives the relative driving power of Leather Belting, with both grain and flesh side to pulley; also, of Rubber, Gutta Percha and Canvass. The pulleys on which the experiments were made were the same in size, on one shaft, and their surfaces severally of leather, polished iron, rough turned iron, and of polished mahogany. The bands were passed over the pulley, one end made fast and stationary, and, on the other, one pound weight was suspended to every square inch contact surface of the band and pulley.

"The number of pounds required to slip the band are given; also, number of pounds strain on the band at which it will cease to slip; and also, number of pounds required to make it continue to slide.

"The belts were in like condition, and had the same contact surface, the same strain, consequently it is easy to determine the relative value of each, for driving machinery, also that of the pulleys..

TABLE B.

	Leather. Grain side to Pulley.			Leather. Flesh side to Pulley.			Rubber.			Gutta Percha.			Canvass.			
	Commence to slip.	Cease to slip.	Slide.	Commence to slip.	Cease to slip.	Slide.	Commence to slip.	Cease to slip.	Slide.	Commence to slip.	Cease to slip.	Slide.	Commence to slip.	Cease to slip.	Slide.	Relative value of different pulleys.
Pulley with Leather surface............	6	2½	10	3½	2¼	7	2½	1½	5	2½	1¼	3½	1¾	1	1¾	52
Polished Iron surface..................	1½	1	9	1¼	¾	6½	1¼	¾	4½	¾	½	2½	1	½	2	33¾
Rough Iron surface..................	1½	¾	3	1½	¾	2¼	1¼	¾	4	¾	½	1½	1	¾	1¼	21½
Smooth turned Mahogany.............	3¾	2¼	4	3	1½	3¼	2¼	1½	4½	2½	1	2½	1¾	1¼	13	36¾
Relative value of each belt..........	45¼			33½			29¾			19¾			15¾			

(To be continued.)

Mechanics, Physics, and Chemistry.

ON ELECTRICITY APPLIED TO REGISTERING VIBRATIONS.

THE laws which govern the vibration of cords or wires, have been obtained by comparing the sounds they produce, with the notes of a syren. Without questioning the accuracy of this method, it will still be desirable to obtain the laws of vibration without regard to the effects which vibrations produce; a *direct registry* cannot fail to be more satisfactory.

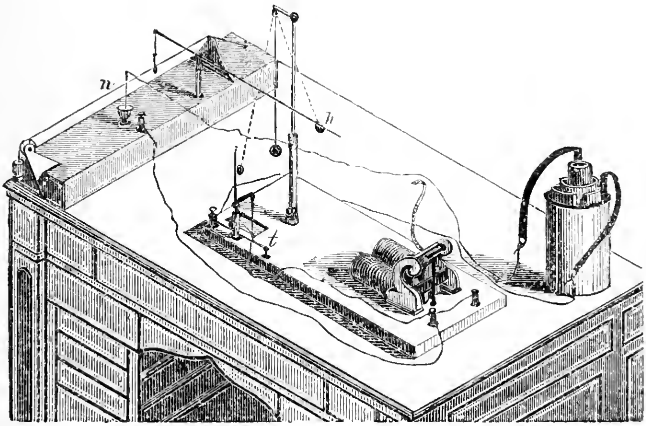
Now it is clear that however rapid may be the vibrations of a cord, the velocity of the electric force is greater; moreover, it is not impossible to make a succession of electric impulses produce a corresponding succession of *permanent* effects, which can be seen and counted, so that if a vibrating body can be made to open and close an electric circuit, the electric force may be depended on to register its vibrations.

The practical questions are, 1st, How shall a vibrating body be made to open and close an electric circuit without having its motion embarrassed? 2d, How, in a legible manner, can the number of these rapid impulses be registered? And 3d, By what means can the time of vibration be accurately measured?

To register the vibration of cords and piano wires, the following arrangement of apparatus has been made. Through the middle point of the vibrating cord, passes a firm cambric needle, *n*, the point of which, will, when the cord is at rest, be very near, but not in contact with the surface of mercury contained in a cup beneath. A galvanic battery is connected, one pole with this cup, the other with a trough, *t*, containing mercury, into which dips the end of a wire bent twice at right angles, and turning freely upon a hinge. To this wire is joined one end of the helix of an electro-magnet, while to the other end of the coil is attached a flexible metallic thread (a ravelling of gilt lace), tied into the eye of the needle, which passes through the vibrating cord. Now, by the vibration of the cord, the needle point will be brought in contact with the surface of the mercury under it at the end of every double vibration, and a current of electricity darts through the wires, magnetizes the electro-magnet, which pulls the armature to its poles, and brings the registering point in contact with the paper. As the

paper is drawn swiftly along by clock work, while the armature with its sharp and soft lead pencil point is in motion, the vibrations of the cord are registered upon the paper in a line of distinct black dots, easily counted.

To measure time, in the present form of the apparatus, a pendulum is used. The pendulum when drawn to one end of its arc, rests against one arm of a lever, *h*, while the other arm carries a pair of pluckers which grasps the cord. A pressure of the finger causes



this finger to release, at the same instant, the pendulum from one end, and the cord from the other. The wire, whose lower end dips into the trough of mercury, can at any time be brought into the arc of the pendulum, by moving the block to which it is fastened, without breaking the circuit, but when this is done, the ball will strike its upper end, and knocking it over, lift the lower end from the mercury, and open the circuit. The motion of the armature begins with the beginning of the first vibration of the pendulum, and stops at the end of one, three or any odd number of seconds, and the number of dots left upon the paper, shows the number of vibrations of the cord.

This apparatus applied directly to the wires of a piano in daily use, gave the following results:

Notes....................	C	C♯	D	D♯	E	F	F♯	G
Vibs. per sec.......	127·5,	117·5,	114·5,	107·8,	101·5,	981,	92·8,	87♯,
	G♯	A	A♯	B	C			
	8♯·8,	76·♯,	74·♯,	67·♯,	64·2.			

Each of these numbers is the mean of a set of experiments, but

in no instance did the numbers in the same set differ by more than a single dot, which must correspond to an error of *less than one-half of one vibration.*

In this set of experiments, however, the adjustments of the apparatus were not such as extreme accuracy would require: in those by which to verify the *laws of vibration* great care was taken that they should be. The two points to be accurately gained are 1st, to adjust the lever, *h*, so that it will release the pendulum and the cord at the *same instant*, and 2d, to adjust the bent wire so that it will break the circuit at the end of the arc of the pendulum.

The first adjustment seemed to be accurately made, as follows: a *steel* wire was fastened firmly at its middle point with one end resting upon the short arm of the lever, and the other bent under the end of the long arm. The ends of this elastic wire must then follow the motions of the lever. By trial, the exact positions of these ends at the time when the ball and cord were released were found, and *at these points* were placed the *ends* of two small wires, which came from the two poles of the battery. A circuit was thus arranged with two breaks, which were to be closed by the motion of the lever. If both are closed at once, then a galvanometer in the circuit will announce the passage of electricity; but unless they are, there will be no motion of the needle. When, therefore, by a sudden pressure on the lever, the needle moves, there can be little doubt that the cord and the pendulum begin their vibrations at the same instant.

The same method secured the second adjustment also. The upper end of the bent wire passed the end of one battery wire at the moment when the pendulum reached the end of its arc. A projecting point, cemented to the lower bend of the wire, passed the other battery wire at the moment when the lower end left the mercury in the trough. If, then, the needle of the galvanometer moves, we know that the two openings in the circuit are closed at the same instant. Still farther, the bent wire is small, and so nearly balanced on its hinge that a single grain weight was force enough to throw it over, while the pendulum ball was of lead, weighing about ten ounces; the wire could not impede the motion of the pendulum.

The apparatus for these adjustments is not a permanent part of the instrument, but it can easily be made so, and then *every experiment would be tested* by the motion of the needle. Only the first adjustment would need this.

It is not to be supposed that this form of the apparatus is the

most efficient that can be devised; experiments indicate, for example, that a *chemical* registry may be used to advantage. Moreover, it seems clear that by this method the vibrations of not only wires and rods, but of other bodies, whose amplitude of vibration is at all appreciable, may be registered. And when we remember how large a place the theory of vibrations holds in modern science, may not the importance of the subject lead other experimenters of greater skill and more abundant resources, to welcome the hints here offered.

THE BEST MODES OF TESTING THE POWER AND ECONOMY OF THE STEAM ENGINE.

By Charles E. Emery.

Late of the U. S. Navy and U. S. Steam Expansion Experiments.

It is unnecessary for us to do more than simply call attention to the extended usefulness of the steam engine. It is the only motor that has successfully competed with or supplanted the changeable and uncertain power derived from animal muscle, and the natural forces of wind and water, and its varied adaptations and applications have brought it into general use throughout the civilized world, not only in stupendous water works and manufactories, and in furnishing reliable and rapid communication by land and sea, but also in reducing the physical exertions of both sexes in the less grand but more important operations of the producing community in the forest, field, and farm house.

Surely, then, the steam engine is not an experiment. Years ago, it was made a success, and soon became a necessity; and notwithstanding the grand discoveries that have been made in theoretical and practical science, the steam engine has to this day remained unchanged in every important particular. The principal advance has been in the perfection and general adoption of the simple high pressure engine. Many of the so-called improvements were mere variations in form and in the details of construction, which often failed to produce as economical results as older well tried mechanism. Nearly all the true improvements have been in workmanship and in adaptations, and applications to various uses. A few of the general principles which influence the economy of the steam engine have long been known, and our manufacturers have in very many cases claimed a superiority for their engines on account of alleged

excellence in the details of the valve gear, or other mechanism, designed to secure the results promised by theory, forgetting that theoretical propositions are of little value, unless all the conditions assumed are the same as those in practice, which is rarely the case. It therefore often happens that engines, which in the opinion of the educated engineer, possess many of the elements considered necessary for economical workings, do not have those elegant, moving details, which fix the attention of the amateur, and delight the eye of the skillful mechanic. Business men seek only to sell, and therefore push into chief importance such points as the purchaser can see and understand.

Statements are made also regarding actual performance, but they cannot be considered impartial, because the trials upon which they are founded are made by interested parties, with no competition present. We have therefore to conclude that the purchaser of a steam engine has to base his selection almost exclusively upon the excellence of simple, mechanical details, and having done this, if the engine works well, and especially if it does better than the old neglected one, with its worn out boilers, he is entirely self-satisfied, and ready to sign a recommendation to the public of the engine which he has selected, thereby benefitting the manufacturer, and flattering his own vanity. But little true progress can be made in this way, as each manufacturer and purchaser knows little more than the result of his own experience.

To bring the steam engine to a high standard of efficiency, accurate, comparative trials should be publicly made of every different system of construction. This would be most satisfactory, if it could be done in the same place, doing the same work, under the same circumstances. This would require the erection of costly experimental fixtures, which could be done by private enterprise for expected gains, or by the combination of several wealthy manufacturers, or, better, still, by some scientific organization. The majority of cases must, however, be reached by trying the steam machinery in the actual performance of the duty for which it has been purchased. We desire, then, in our present inquiry, to ascertain methods and means to test the power and economy of the steam engine in a strictly scientific manner, which shall be above criticism, and *also* under the practical circumstances of every-day use.

We propose first to mention some of the *terms* in general use on the subject, then to discuss the ways and means employed to measure

the power and its cost, and afterwards to select proper units of comparison and point out the manner of their practical application.

A steam engine is simply a *heat* engine. The *heat* evolved by the combustion of fuel is imparted in the boiler to *water*, separating and agitating its molecules, and thus forming *steam*. The steam exerts *pressure*, varied according to its density, upon all sides of the vessels in which it is enclosed. This pressure or *force* is measured in *pounds* per square inch. The elastic force of the steam acting upon the engine piston, produces *motion*, which is measured in feet. The combined effects of *force acting through distance* produce mechanical *work*, which is measured in *foot-pounds*. The number of foot-pounds which an engine is capable of developing in a given *time*, expresses the power of the engine. The unit of the power is one *horse-power*, the value of which is conventionally fixed at 33,000 foot-pounds per minute.

In proportioning steam machinery for any particular purpose, the first thing to settle upon is the amount of power required, and this being fixed in all cases, within certain limits, *the practical question is to obtain a certain power at the least possible cost.*

We will first discuss the ways and means used to measure and determine

The Power.

As has been said, the power of an engine depends upon the *work* done in a given time, and as work implies *force* and *motion*, we must ascertain three things before we can calculate the power, viz.: the *mean force* and the *distance* through which it is exerted, also the *time* required for the movement. Having these, we first ascertain the distance moved per minute, and this, multiplied by the mean force, gives the number of foot-pounds per minute, which divided by 33,000, gives the *horse-power*. The distance through which the force is exerted, is usually calculated from the number of revolutions made per minute by the engine, which can be ascertained approximately, by actual count, but better by means of a register. The speed of the engine is varied more or less by every change in the load, or in the pressure of steam, even when a governor is used; for a change in speed *must* take place before the governor can operate. The variations are small, with sensitive regulators, but in a majority of cases, would materially affect the result. The true plan, then, is to attach a register to the engine, the indications of which

should be taken once an hour, to check mistakes, and in the calculations the revolutions per minute should be an average for the whole time through which the trial extends. If the power is to be calculated from the pressure on the piston, the piston movement is also used and ascertained by multiplying the revolutions per minute by double the stroke of the engine (when the latter is double acting). When the tension of a belt or series of springs is to be used in calculating the power, the movement of each must also be found, and must be calculated from the speed of the engine. It will thus be seen that two elements of the *power* are easily ascertained, viz.: the *time* and the *distance* through which the force is exerted. The mean driving force is more difficult to obtain. There are two instruments in use for measuring this, viz.: the *indicator* and dynamometer. These two names are used in this paper in a restricted sense. The first is applied only to the well known steam engine indicator, and the latter to that form of dynamometer which is used to measure the force transmitted by revolving wheels or shafts.

It would be impossible, in the limits of this paper, to give a detailed description of the indicator. We therefore will mention only such features as are necessary to explain its mode of operation. The indicator is so constructed and attached that steam from the main cylinder presses upon one side of a small piston in the instrument, the atmosphere pressure being upon the other side. To the indicator piston is attached a spring and a pencil, the latter arranged to mark on paper. The predominating pressure on the indicator piston, whether of the steam or the atmosphere, extends or compresses the spring in proportion to the intensity of the pressure, and moves the pencil up and down on the paper. The paper is arranged on a drum, which is so connected that it has a side motion corresponding to that of the engine piston. Consequently, as the engine piston moves, the *paper* is moved *sideways*, and as the pressure changes, the *pencil* is correspondingly moved *up and down*, so that the figure or diagram, traced on the paper, is a combination of the two movements, and should show the pressure at each and all points of the stroke. The mean of a number of ordinates on the diagram, represents the mean pressure per square inch of piston, which multiplied by the area of the piston, gives the total face, which produces the piston movement, from which the power may be calculated, as has

been before explained. The indicator is a beautiful instrument, of such great value to the steam engine, that it may be said to deserve the numerous words that have been spoken in its praise. Still, in many cases, where it has hitherto been considered practically perfect, its indications are of the most deceitful and unreliable character. It shows very perfectly whether the valves are adjusted properly, and often when applied to an engine, which is working improperly, a mere glance at the diagram will reveal the difficulty, and suggest the remedy. Large leaks in the valves or piston may also be detected in this way. The indicated pressure at the end of the stroke has very often been employed to determine the quantity of steam used by the engine. Calculations founded on such a basis are entirely worthless, as will be explained, when treating of the cost of the power. It has often been attempted also to calculate the friction from the indicator-friction diagrams, but the system is practically erroneous, as will be explained hereafter.

The indicator is chiefly employed, however, to determine the power of an engine, it being supposed that the diagram shows correctly the pressure at all parts of the stroke. Even this it fails to do under certain circumstances. The moving parts of the instrument must have weight and friction, and some force is necessarily required to overcome the latter and put the mass in motion. If, therefore, the pressure be ascending, the indicator will show less than it should, and when the pressure is descending, the instrument will show more than it ought. In either case, then, the length of the ordinates is increased during any change of pressure, whence the mean pressure indicated is greater than actually existed in the cylinder. Until quite recently, we supposed that these inaccuracies were too small to require serious attention. Experiment has, however, proved the contrary. The Richard's or "parallel motion" Indicator is undoubtedly a great improvement upon the old style; but using one of the best of the first named instruments made by Elliot Brothers, of London, and carefully adjusted so as to move freely, but without shake in the joints, we have found inaccuracies in the diagrams of from 10 to 25 per cent. The results were so remarkable and unexpected that we propose to point out ready means whereby anybody may repeat the experiment. As has previously been explained, the weights and friction of the moving parts of the indicator cause the pencil to be somewhat tardy in re-

cording the *changes* of pressure, *hence the greater the extent or the rapidity of the changes, the greater should be the discrepancies.* If an engine be working full stroke, the steam and exhaust lines of the diagram will change so little that there will be *time* for the piston of the indicator to adjust itself to the pressure; the contrary will be the case, however, when the steam line is broken by expansion, or the exhaust line by extreme cushioning. The discrepancies would increase also with the speed of the engine. To test the amount of the variations, select an engine running at least 50 revolutions per minute, and provided with a good adjustable cut-off. Make arrangements so that the load of the engine will be as uniform as possible for a little while. Then cut off the steam in the cylinder as short as is possible to keep up a certain speed, then count and record the revolutions per minute, and take indicator diagrams for a short time. After this, without in any way altering the load, change the cut-off to full stroke as nearly as possible, and adjust the throttle so that the engine will make exactly the same speed as before, and again take indicator diagrams. The operations may be repeated several times to allow for inequalities in the load. The indicator diagrams taken under such circumstances, though of different shapes, should of course show the same mean pressure; for the engine was developing the same power in all cases. In practice, however, the cards taken with a short cut-off, will have a much greater area than the others, so that in fact the difference can be readily detected by the eye. We conclude, from our experience on the subject, that the indicator cannot be depended upon to accurately measure the power of high speed engines, working expansively. In many cases in practice, we suppose, however, that the discrepancies are so small that they may be disregarded. In marine practice, for instance, the paddle-engines run very slowly, and screw-engines do not generally work at a high degree of expansion; and in general, the power of all engines running slowly, or with little expansion, may be measured by the indicator with sufficient accuracy for general comparison. The difficulty occurs in cases like the locomotive or in stationary engines working very expansively, at a speed of from 50 to 200 revolutions per minute. In these cases, the indicator should be depended upon only in comparative tests, where the engines run at about the same speed, with about the same pressure of steam and degree of expansion. The stiffer the spring of the indicator, the lighter the moving parts, and the

smaller the range of the motion, the smaller will be the variations. Gooch's locomotive indicator seems to fulfil these requisites the best of any yet designed. The indicator is often applied to both steam and pump cylinders of pumping engines, when the difference in the power thus obtained shows the friction of the machinery.

The measurement of the power in the steam cylinder by the indicator is defective also, because it takes no account of the friction of the engine. If all engines of the same power, worked with substantially the same friction, this last consideration would be of little or no consequence. But a multiplicity of parts, awkward proportions, improper fittings, weak framing, &c., may cause some engines to have far greater friction than others. Again, questions as to the proper size and speed of an engine are influenced by friction. For instance, if a large engine is more economical than a small one, will not the gain be balanced by increased friction? The only way of settling these questions is by measuring the *nett power* of the engine, or that which *is available for useful work.* This can be done by the *dynamometer.* This instrument is made in many different forms. The friction dynamometer consists substantially, of half clamps, or boxes fitted to a revolving shaft, and kept from turning therewith by a lever held in position by weights and a spring balance. When in use, the clamps are tightened until they create sufficient friction to absorb the powers; the weights are then adjusted till they nearly balance. The amount of weight, the tension of the spring, and the speed of the shaft are then noted, when the power transmitted through the shaft may easily be calculated: for the force of the weight and spring is multiplied by the lever in proportion to its length, divided by the radius of the shaft, and this multiplied by the velocity of the bearing surface is — per minute, gives the foot-pounds. This form of dynamometer is little used, because it absorbs instead of transmitting the power. Besides, it is difficult, on a large scale, to maintain a constant friction for any length of time.

The dynamometers of greatest practical value transmit, and at the same time indicate the power, without in any way interfering with the regular duty of the engine. For instance, if the power be transmitted by means of a belt, and we can in any way measure the tension of the two parts, their difference represents a force moving with a given velocity, which may easily be reduced to units of power.

A dynamometer on this principle has been used abroad, which was re-invented by Horatio Allen, Esq., President of The Novelty Iron Works, in this city, and by him applied to the engines used in the U. S. Steam Expansion Experiments. In this case, the driving and driven shaft were separate, but lay in the same horizontal line. Near the contiguous ends, large wheels were placed with a **V** groove in the circumference of each. An endless rope passed in both directions, over the top of one wheel, then under side pulleys over the top of the other wheel. The side pulleys were below the centre of the large wheels, and were of such size that the four parts of the rope leading to them hung vertically. These pulleys ran in bearings free to slide vertically, and were connected to platforms carrying adjustable weights. The motion of the wheel, on the engine shaft, turned the other shaft in the opposite direction, by means of the rope, but tended, at the same time, to lift the side pulley. The opposite side pulley was weighted sufficiently to keep the rope from slipping, and weights and a small spring were adjusted on the driving side to balance the lifting force. Then half the difference in weight on the two side-wheels equalled the tension of the cord, or the driving force, which, together with the velocity of the cord, furnished the only elements necessary to calculate the power. This instrument had means attached to automatically record the strain on the cord, and answered its purpose very perfectly and satisfactorily. It was, however, too expensive and cumbersome for every-day use. Three beveled wheels, on the above principle, have been used as a governor, and would doubtless make a good dynamometer also.

Steel springs, properly arranged, form, we believe, the best dynamometer for practical use. As commonly constructed, a pulley, through which the power is transmitted, is made loose on the shaft and then is driven from it, through the intervention of springs; or one shaft is driven from another in the same manner. It is necessary, then, in order to calculate the power, to ascertain the tension of the springs, and their velocity where the force is applied. Neer's dynamometer, on this principle, may be taken as a type of its kind, and has given general satisfaction. The instrument must be attached in two places: one part to the driving shaft, and the other to the pulley or shaft to be driven; and the latter must not receive any motion except what is transmitted through the springs of the instrument. Secured to the instrument are two or more coiled steel

springs, lying in the same direction as the shaft. A chain passes through each spring, around a pulley, and from thence to a circular hub on the other shaft or pulley, to which hub the end of each chain is secured. Now if one shaft is moved, it tightens the chains and compresses the springs sufficiently to overcome the resistance and put the driving shaft or pulley in motion. The longitudinal motion of the springs moves a hand on a suitable dial, which is graduated so as to show the horse-power, when the shaft makes 100 revolutions per minute. The exact force is found by counting the speed of the shaft, and correcting the reading accordingly. The minor details of construction can best be explained by the manufacturers. The accuracy of an instrument of this kind can easily be tested by weighing the springs, measuring the distance from the centre at which they act, and correcting the dials accordingly.

(To be continued.)

ON THE EXPERIMENT OF LISSAJOUS.

By Prof. Edward C. Pickering.

In 1857, M. Lissajous studied the curves produced in the following experiment. Mirrors are attached to the prongs of two tuning forks, whose planes of vibration are at right angles to one another. A ray of light falling on the first, is reflected from it to the second, and is then projected on a screen by a lens. If, now, the fork which vibrates in a horizontal plane is sounded, the motion thus imparted to its mirror causes the luminous point on the screen to describe a horizontal line. Sounding the other fork in the same way, produces a vertical line. When both vibrate at the same time, various curves are produced, dependent on the relative pitch of the two forks.

When showing this experiment to an audience, it is desirable to have a set of curves, drawn on a large scale, for comparison. As, however, their geometrical construction is somewhat laborious, I have devised a machine, represented in the adjoining photo-lithograph, by which they may be drawn mechanically.

The paper on which the curves are drawn receives a horizontal motion to and fro, while, at the same time, the pen is moving vertically up and down. These motions are imparted thus:—Two

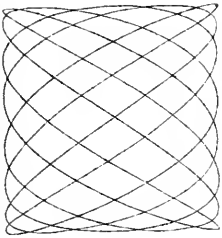
wheels (Fig. I.), connected by a belt, carry cranks, of which the lower one moves in a vertical slit in the rear of the square board to which the paper is attached. This board is kept in place below by a rod, over which it rolls, and above by a guide of sheet-brass. If, now, the lower wheel is turned, the paper moves horizontally through a distance equal to twice the crank-arm. A similar motion is imparted to the pen by a horizontal slit, in which the upper crank slides, and by which the pen is moved up and down. The rod is made flat to prevent its turning; or, it may be made circular, if we fix its slit in guides.

As a belt connects the two wheels, turning one moves both pen and paper. All the different phases of the curves corresponding to any given ratio of the number of vibrations of the two forks, may be drawn by merely sliding the belt over one of the wheels. As the ratio of the diameter of the latter corresponds to that of the number of vibrations of the forks, a series of wheels of different sizes are made, which, when combined two-and-two, give an almost endless variety of curves. Perhaps the best ratios, if we have four wheels, are $1 : 1\frac{1}{2} : 1\frac{3}{5} : 2$, by which we get curves corresponding to $\frac{16}{15}$, $\frac{5}{4}$, $\frac{4}{3}$, $\frac{3}{2}$, 2 and $\frac{8}{5}$, or to the semitone, major third, fourth, fifth, octave and minor sixth.

It may be proved that the curves coincide with those of Lissajous, since the angles described by the cranks are proportional to the diameter of the wheels; also, the distances of either pen or paper from the middle points of their paths are proportional to the sines of these angles. In other words, calling v the angle traversed by the upper crank, a, the ratio of the two wheels, and B, an angle dependent on the phase of the vibration, we have $y = \sin v$, and $x = (av + B)$ equations defining Lissajous' curves.

To obtain curves corresponding to every ratio of the forks, expanding wheels may be used, in which, by turning a spiral guide, six sectors are made to approach or recede from the centre. With two such wheels, whose diameter can be altered from 3 to $4\frac{1}{4}$ inches, and from $4\frac{1}{4}$ to 6 inches, all the curves within the limits of an octave can be drawn. By a set screw the crank-arms may be shortened so that smaller curves may be drawn, also those inscribed in a rectangle instead of a square.

To draw the curves in ink, it was necessary to devise some new kind of pen. That represented in Fig. V*a*, answered as well as could be desired. It is easily made from a glass tube, by thicken-



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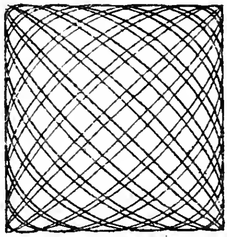

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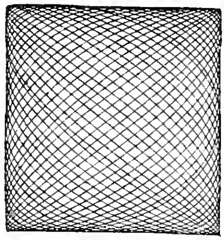

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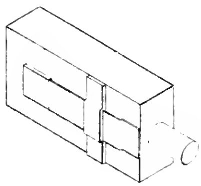

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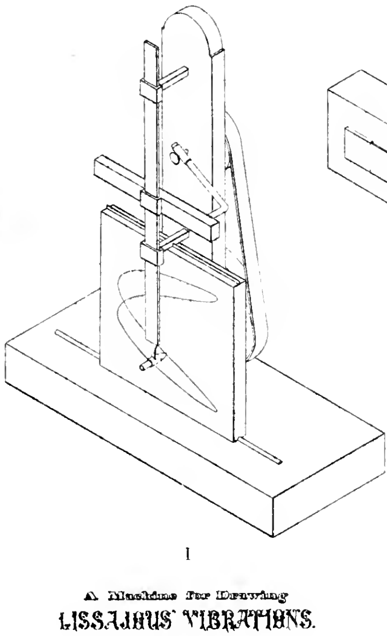
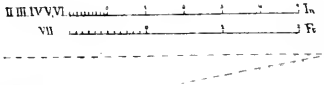


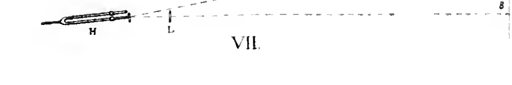
I

A Machine for Drawing

LISSAJOUS' VIBRATIONS.

BY E C PICKERING





VII.

ing and nearly closing one end with the blowpipe, placing a drop of water in it, and grinding it flat on a file. When filled, the convex liquid surface at the small end retains the ink, while, when pressed on the paper, the flow takes place with great smoothness. Fig. V*b*, shows a modification which is useful for drawing diagrams. The pen is made so as to contain a large supply of ink, which is kept from running out by closing one end by a cork, through which passes a tube, as in the figure. The pressure is then constant, and equals that of a column whose height is the distance from the point of the pen to the end of the inner tube. By varying this distance, we can use a high pressure for thick ink, and a low pressure for that which is more fluid.

In Figs. II., III. and IV., are given specimens of curves drawn by the apparatus above described, to show its practical working. In Fig. II., the ratio is 5 to 8, or the curve of the minor sixth; in Fig. III. it is 15 to 16, or a semitone; and in Fig. IV. about 3 : 4, more nearly 44 : 59, or a sharp fourth. In the latter case the curve does not close exactly, since the ratio is incommensurable. As these curves are here reproduced by photography, the slightest errors are visible, and they furnish a good test of the workmanship of the machine. Probably still greater accuracy would have been attained had the apparatus by which they were drawn been constructed by a professional instrument-maker.

When projecting the curves of Lissajous it is somewhat difficult to show the more complicated forms, owing to the large size of the luminous point compared with the curves themselves. I find the arrangement of Fig. VII. gives the best results. A is an aperture through which a ray of sunlight enters; V and H are the two forks, one vibrating in a vertical, the other in a horizontal plane; L is a lens of 6 feet focus, by which the curve is projected on the screen, S. The size of the curve depends only on the distance, L S, or on H S, if the light traverses the lens before meeting the mirror, H, and, therefore, the lens must be brought near the aperature, A. But, if we use a lens of short focal length, the luminous point is greatly magnified, since its diameter is proportional to $\frac{\text{L S}}{\text{A V} + \text{V H} + \text{H S}}$. I therefore use a lens of 6 feet focus, and remove V to a distance of $3\frac{1}{2}$ feet from H, so that A V + V H + H L and L S shall be conjugate foci. This would be the case in Fig. VII., when L S is 18 feet, or in a room

20 feet wide. It is not represented in its true size, in the figure, for want of space.

Lenses suitable for these projections of any size not exceeding 8 inches in diameter, and 6 feet focus, may be obtained at a low price from the makers of cosmoramas. Although not achromatic, their aberration is exceedingly small, from their great focal length. They are useful for many purposes, particularly in large lecture-rooms; thus, in projecting the solar spectrum, I find them preferable to much more expensive achromatic lenses of short focal length. Again, by using one as an object-glass, a very good telescope for class purposes may be made for a few dollars, capable of showing all the more familiar astronomical objects, such as satellites of Jupiter, ring of Saturn, prominent nebulæ, &c., and which would, I think, be of great use to many institutions where a more expensive instrument could not be afforded.

Fig. VII. represents a substitute for the tuning forks in the experiment of Lissajous. It consists of a rectangular box, in which is an aperture just filled by a square plate, carrying the mirror. This is attached to an elastic strip of metal, which vibrates when air is forced into the box, like the tongue of a melodeon-reed. To vary the rapidity of vibration, the metal strip or tongue passes through a guide, by which means its length may be altered, and this may be done with the utmost precision by a screw not represented in the figure. This adjustment is not necessary in the reed replacing the second tuning fork, since a range of more than an octave is readily obtained by the method here described. The advantages claimed for this apparatus over the tuning forks are, that the angular amplitude is much greater, hence the curves on the screen are of very large size; that they may be maintained indefinitely by keeping up the supply by the bellows, and most important of all, by merely turning the screw, the progressive changes of one curve into another may be shown, so that, in a few moments, all possible combinations may be exhibited.

My experiments with this apparatus are not yet completed, as I find that the vibrating part must be made with a good deal of care, or the curve will not be perfect. In fact, in a preliminary experiment, in which I used too feeble a spring, I found that it answered admirably for showing the difference of quality of different sounds. By touching the mirror with the point of a knife, so as to produce a rattling noise, I obtained, with a single reed, instead of a right line, a complicated curve, which varied with every change in the sound.

ANTI-FRICTION ROLLS AND CAGES FOR SHEAVES.

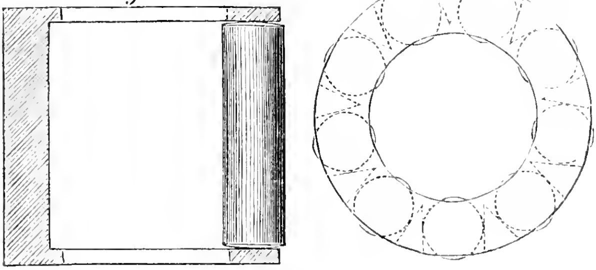
By J. H. Cooper.

In Gregory's treatise on Mechanics, published in London, in 1806, mention is made of Garnet's anti-friction rolls, which consist of a series of rolls arranged parallel to and around about a shaft or pin, each roll having journals fitted in end rings, which latter are secured together, forming a rigid cage for all to revolve in, free from contact with one another, and for preserving their parallelism with the shaft. This appears to be the usual plan of making anti-friction rolls and cages, as it is the oldest, and has the merit of allowing the greatest number of rolls in the circle, as they are placed as closely as possible without touching, but are open to the objection of weakness in the cages from lack of room between the rolls for securing the rings, unless, as is sometimes done, every second or third, or fourth roll, is drilled through for connecting studs to the opposite rings, which answer also for journals for the tubular rolls thus made.

This plan strengthens the cage, but renders the tubular rolls liable to collapse under heavy pressure.

Another old plan, which is probably the simplest, as well as the cheapest to manufacture, consists of solid rolls without journals

Fig. 1.



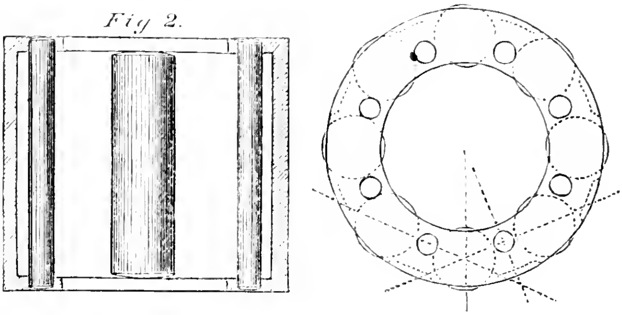
placed in stalls formed by the connecting bars of the end rings, which are cast in one piece with the latter. The proper shape of the stalls is that in which the opposite sides are parallel planes, as shown in Fig. 1.

In Fig. 2, a different system of anti-friction rolls is shown, which

we have devised for the purpose of gaining a better use of this ingenious and useful mechanism.

In this, the rolls are all solid, and are without journals, the bear-

Fig 2.



ing rolls being separated by smaller ones, terminating in the end rings. These smaller rolls have slotted bearings, which allow for slight displacement of the larger rolls, resulting from wear, they occupy a position in the system on a line joining the centres of two contiguous rolls, and have such a diameter as will give them sufficient strength, and cause all the rolls to touch one another when working.

The end rings are united in one piece by bars cast with them, occupying the interspaces of the larger rolls, and outside of the smaller ones, but not in contact with either, thus making a comparatively cheap and rigid cage.

In this anti-friction device, the *contact is all rolling*, and must, in consequence, not only have a minimum of friction, but of wear also.

We have a number of rolls made after this system, in use in the sheaves of cranes, which are subjected to the severe usage of steam power in a pipe foundry, and know them to operate freely and well, and to promise fairly for continuance in good service.

Philadelphia, November, 1868.

EDUCATIONAL

LECTURES ON VENTILATION.

By Lewis W. Leeds.

Second Course, delivered before the Franklin Institute, during the winter of 1867–68.

(Continued from page 424, Vol. LVI.)

From the crude facts that have been thus gathered, we are led to suppose that the proportion of carbonic acid in the exhaled breath, varies greatly under many different circumstances.

Temperature has a very great effect. Experiments have been tried with birds and animals, showing that there is less than one-half exhaled when breathing a temperature of from 86° to 106°, than there is when breathing air at a temperature near the freezing point.

The *proportion* of carbonic acid contained in the exhaled air varies greatly, as also does the aggregate amount; it constitutes from 3 to 8 per cent. of the volume exhaled—probably 4 or 5 per cent. would be the proper average. Hence, as pure air (so called), seldom contains more than five parts of the gas in ten thousand, it follows that the breath contains one hundred times the natural quantity found in the atmosphere.

That jar, therefore, contains about one hundred times as much of this poisonous deadly gas, as does pure air, and not only that, there are particles of my body, there are portions of my brain (I don't mean in the sense in which we use it to express the result of thought), but I mean the actual worn-out material; and the only valuable substance it originally contained—the oxygen—has been taken out. It is upon the whole a very disgusting mass, as you must admit, and we will remove it carefully, so as not to permit it to escape into this room. I perceive you seem a little amused. I suppose it is at the idea of carrying out that one jar of foul air such as we are all the time exhaling into the same air we *inhale* from; but I think this gives you the true idea of ventilation.

The actual amount of air breathed is comparatively very small; it is only about the one-fourth of one cubic foot per minute. But it is the contamination of the remaining mass that causes the trouble. For instance, as the exhaled breath contains one hundred times as much carbonic acid as pure air does, hence, if one cubic foot of breath was diffused through one hundred cubic feet of air, it would add, even to that very large amount, nearly double its normal quantity.

In determining what amount of fresh air must be supplied for each individual, in a room in which we are each breathing into and from the same general mass, it becomes a question of what proportion of other persons breath we are *willing* to take into our own lungs. If we breathe but the one-fourth of one foot per minute, a supply of twenty-five cubic feet per minute would give us one hundred times the actual amount of air that passes through the lungs, so that we should then only have to breathe double the normal quantity of carbonic acid.

This twenty-five feet per person per minute, however, is considered by many a large allowance. As low as ten cubic feet per minute for each person is considered by others to be a sufficient quantity, yet even that is much more than vast numbers of our buildings are supplied with, as may be readily judged from the disgusting foul odors noticed in many of them, and which could scarcely ever be perceived if this small allowance even was carefully furnished. Of excretions from the surface of the body in the form of insensible perspiration, which are constantly occurring, I have as yet made no allusion.

But now suppose that instead of discharging the breath into the general reservoir, we could discharge it into a closed vessel, as we have done, or by a speaking-tube—if I might so term it—or better, a *breathing* tube, directly into a foul air duct, to be carried entirely out of the building; don't you see that one-tenth part of the air that would otherwise be required, would in that case be entirely sufficient? It would then be certainly much preferable to our present arrangements, because we should have radically *pure air*, instead of at best, that which was slightly contaminated.

I don't exactly mean to advise that we should each one carry a long breathing tube in his pocket, and the moment he came into the house, place one end in the fire-place, and put the other in his mouth before he began to talk to you; but I think it demonstrates

the true principle upon which we should act, in making our arrangements for ventilation, which is to say—to confine the polluted air—and to remove it as soon as possible from the room. I design showing you in a subsequent lecture that this has been my guide, in the arrangements for ventilations in hospitals, churches, schools, etc. and which I may add have been very successful.

It is a curiously interesting fact, that the temperature of the body remains nearly uniform at 98°, under the most extreme variations of external temperature, say from 40° to 60°, below zero, as experienced by Arctic voyagers, to a temperature of from 200° to 300° *above*, experienced by persons accustomed to entering and remaining for some time in furnaces for baking certain wares, and in like employments.

The cooling off, when we are very warm, is caused by the vaporization of the perspiration. A pint of water makes 1,700 pints of steam, but as it turns into vapor, or is enlarged like the expansion of a sponge, it absorbs heat very rapidly. Thus, when you perspire freely, or when you see the vapor arising from a hard-worked animal, you may know that the heat is being conducted away rapidly. It is for this reason that the drinking of a cup of hot tea on a warm summer day, by inducing profuse perspiration, has such a cooling and refreshing effect.

But this very fact of profuse perspiration, is frequently the cause of what we term "colds;" because, when we are perspiring thus freely from active exercise, and the heat is escaping very rapidly from the body, we are apt, when we have an opportunity to rest, to indulge in the agreeable process of "cooling off," wherein the temperature of the body is reduced so much more rapidly than in its ordinary conditions, that almost before we are aware, it is quite below the usual temperature of health. And so it happens that the relaxation of the respiratory organs, and the cessation of the rapid flow of blood, prevents the quick renewal of the heat; or in other words, the fires having been put out by the profuse perspiration, it requires a long time for them to be rekindled, and to get to burning again freely.

There are said to be 2,800 little tubes for the escape of the perspiration, in every square inch of one's body, and that the united length of all those in the skin of a single person, is about twenty-eight miles; so that the length of those in *two* persons, would reach from here nearly to the Atlantic Ocean. Now, the little valves at

the outer ends of these tubes, close very quickly on being exposed to the cold. But as the flow of the current from the interior of the body, does not feel the effects of the cold as suddenly as the valves, it will continue to press forward with its load of impurities, until, being checked from escaping, it will be dammed up near the ordinary places of exit, causing a pressure or inflammation.

To show what powerful effects may be looked for by this sudden stoppage of perspiration, we need only to adduce the fact of the large amounts of moisture which may be driven from the body through the medium of these channels of the skin. A number of experiments tried by Dr. Southwood Smith, on the men working in the Phœnix Gas Works, showed that the average loss of weight per man while charging the furnace or during an exercise of about fifty minutes, was something like three pounds. Just think what an inflammatory effect this enormous pressure, this sudden restraining of the great flow of impure moisture, must produce!

And it is not on the *external* surface only, that this result is produced, but the lining membrane of the air-passages to the lungs is affected even more suddenly, and is also more sensitive than any other. Therefore, we should always remember that the moment we diminish any active exercise, we should immediately, even while much heated, put on an additional garment, or otherwise provide for a very gradual cooling off; being careful that we first cool off *internally*, before allowing the temperature of the external surface to be much lowered.

The manner of taking cold on leaving a crowded and badly ventilated room, is quite different.

You remember the experiment with the flies that lived ten times as long in the foul air we breathe, as did those in the pure air, and which we attributed to the very sluggish action of all their living functions. Now, when you leave a hot, foul room, although your perspiratory glands may be fully open, and there may be a rapid vaporization from the surface of the body and the air-passages to the lungs, which would not be caused by exercise in this case, but, worse, by the heat of the room, yet the vitiated air that you have been breathing, has not only choked up your lungs and filled your body with the actual presence of much poisonous material, but it has so reduced the circulation that you cannot get up a reaction.

As in one minute after you have emerged into the pure air, you have breathed twenty times, or inhaled 400 cubic inches, the lungs

therefore, are the parts most quickly affected. You will frequently feel the inflammation thus produced in your throat, in five or ten minutes after leaving such a room; but you must remember that in ten minutes you will have breathed 4,000 cubic inches of cold air. To leave a room such as I have spoken of, and, in place of walking, to sit down in a cold, crowded, foul car, with your back towards the window, is one of the very best possible means, not only of taking cold, but of contracting any other disease to which you may possibly be subject.

(To be continued.)

SUNLIGHT AND MOONLIGHT.

A Lecture delivered at the Academy of Music, before the Franklin Institute, on May 23d and June 6th, 1868.

By Prof. Henry Morton, Ph.D.

(Continued from p. 418, Vol. LVI.)

We will now pass from the subject of reflected to that of originally emitted or developed light—from the Moon to the Sun.

The most perspicuous method of treating this subject will be to explain, in the first place, one of the most probable among the modern theories of the constitution of the Sun, and then to exhibit the various facts or phenomena on which the theory is based, and from which it receives its support.

We believe that the Sun consists of a dense central mass, composed of only the most refractory materials, intensely hot, and probably fluid, at its surface at least, with heat. From this supernatant ocean of melted matter, rise, continually, streams of vapor, which, at a certain distance, begin to condense, through loss of a part of their heat by radiation into space, and form clouds of minute liquid particles, intensely hot, and therefore luminous in a high degree.

These clouds form the visible surface or photoscope of the Sun, and are composed of such elements only as will endure an intense heat without vaporizing. Above this cloud region, extends another, which may be called the true atmosphere of the Sun, being composed of such elements as are permanently gaseous under the conditions of high temperature there existing. Such substances as

Sodium, Magnesium, Calcium, Iron, are believed to exist as permanent vapors in this region, as well as Hydrogen and other gases. Into this region are occasionally projected denser aggregations of similar vapors, as of Sodium, Magnesium and Iron, which float, for a time, as clouds of ignited gas, before diffusing among the surrounding mixed atmosphere.

Figure 1, in the accompanying plate, will give some general idea of this arrangement of parts or regions. While Figure 2 will convey some notion of the detail implied in part of this action, this drawing being intended to represent a view at the surface of the Sun, showing the molten ocean, the uprushing streams of vapor, the cloud-caps at their heads, and the in-rushing meteorites, to be presently noticed.

These clouds, it may be here remarked, are, on the average, about 1,000 miles broad each, which shows us that here, as in our lunar landscapes, we are dealing with distances which it is far easier to name than to imagine.

As a cause and supplying source of this intense heat which we have here assumed, we believe there exists the following action: The space of the solar system is filled with a vast number of ponderable objects, individually minute, but, in the aggregate, considerable. Some of these, from time to time, fall upon our planet, and are then called Meteors; they are, however, much more densely aggregated as they approach the Sun, and, by mutual interference and disturbance, will be constantly losing their orbitual directions, and, under the influence of gravity, falling in upon the solar mass.

The velocity which bodies would acquire in free space, under the influence of the Sun's attraction, is easy to calculate, and would be, under different assumptions as to original distance, &c, included between the limits of 60 and 85 miles per second.

We know what happens to a rapidly-moving body when its motion is arrested by some obstacle—it becomes heated. Thus a hammer-face is hot after striking a dozen blows; and a cannon-ball, arrested by an iron target, becomes almost red-hot; so a flint strikes off particles of steel, and ignites them by its arrested motion. But if *these* motions and velocities do so much, what must such as we have named develop? A cannon-ball has a velocity of 1,500 feet per second, or about one-third of a mile; it almost grows red-hot by its own impact; what heat would it develop if its velocity

were 60 or 80 miles—that is, 180 to 255 times as great? We can readily calculate this, and find that the heat which any mass would so develop by its blow, would be from 4,600 to 9,200 times as great as if it were a mass of coal consumed in oxygen.

We see, then, that this impact of falling bodies, under the existing conditions of the question, would be a far more plentiful source of heat than any other which we could suggest; but it may yet be asked, What evidence have we that such masses of matter are in existence, and are rolling in upon the Sun?

The zodiacal light is the first proof of this supposition.

In the evenings of March and April, in this latitude, when twilight is of shortest duration, we may see, to the west, after sunset, a conical mass of light rising into the sky. This is the zodiacal light. And, again, in September and October, before sunrise, a similar appearance is observed. As we approach the equator, where, as we know, the twilight is always brief, the zodiacal light becomes more distinct, and visible throughout the year, on clear nights; it is also more vertical, and appears, in fact, to lie in the plane of the ecliptic.

The most natural hypothesis as to the cause of this appearance is, that there are here shown to us a cloud of minute bodies revolving around the sun, illuminated by his light. Variations in the apparent magnitude and intensity of the zodiacal light, point to changes in the conformation of this swarm of minute planets, such as we cannot but anticipate from their individual minuteness and close juxtaposition; and agree entirely with the supposition that part of them are, from time to time, falling in upon the Sun, and that they are also receiving supplies from the exterior space, as we shall next see, for the next proof of the supposed origin of solar heat is furnished by the *Meteors*. We need not here explain what is meant by a meteoræ rolite, or falling star; there can hardly be a person present who has not seen many of them; suffice it to say, that there is no shadow of doubt that these are small bodies of matter which, moving through space, either encounter our atmosphere alone, and flying through it at vast velocities, are heated to ignition by the resistance it offers, or running against the solid mass of the earth, are arrested as well as heated by the encounter.

By a careful observation of the velocities and directions of these bodies, when thus momentarily exhibited to us, as well as the times of their appearance, and the position of the earth in her

orbit, at such times, it has been proved that there are a great number of streams, rings, or elongated cosmical clouds of such bodies sweeping in, to and around the Sun, from all parts of space. Through these streams the earth passes at certain seasons, and we then see a shower of shooting stars, while at all other times we can have no knowledge of their presence, except so far as some of them may be combined with, and add to, the luminosity of the zodiacal light.

It would appear from the observations of Schiaparelli and Newton, as well as of others who have given attention to Meteoric Astronomy, that the earth's orbit intersects at least thirteen of these streams, among which, some, as that of the November Meteors, are well-defined and fully known as to the limits of their orbits, rate of motion, extent and date of first entrance into the solar system, &c.*

Other groups are of a more irregular description, and may, indeed, be regarded as being *cosmical rivers* of the scattered matter left unaccumulated on the boundary-line between great attractive centres, in the original formation of suns or stars, from the nebulous condition of the universe: and as afterwards drawn down towards some preponderating centre, but dragged out into a long trail, displaced, bent and twisted by the attraction of planetary or other masses, and so sweeping in towards and around the Sun, in sinuous lines, in which condition from their vast length, centuries may be occupied in their passing any one point.

The mutual interferences in motion, and consequent in-fall upon the Sun, of masses so grouped and thus moving, must be constant: and, indeed, in at least one remarkable case, we have palpable evidence of such an action. In December, of 1845, Biela's comet was seen to be divided in two, and, on the date of its calculated return, in 1865, it had disappeared entirely. Now, at each of these periods the comet appears to have passed through the orbit of the Meteors of November 27th. whose direction of motion is retrograde, while that of the comet is direct; and it would thus seem that the resistance offered by the innumerable minute particles of the meteoric stream, had first divided the comet, and then caused it, together, no doubt, with these conquering atoms, to fall into the Sun.

* For a full account of these subjects, we would refer to this *Journal*, Vol. LIV., p. 14, or to Professor Kirkwood's excellent little book on Meteoric Astronomy, published by J. B. Lippincott, of this city.

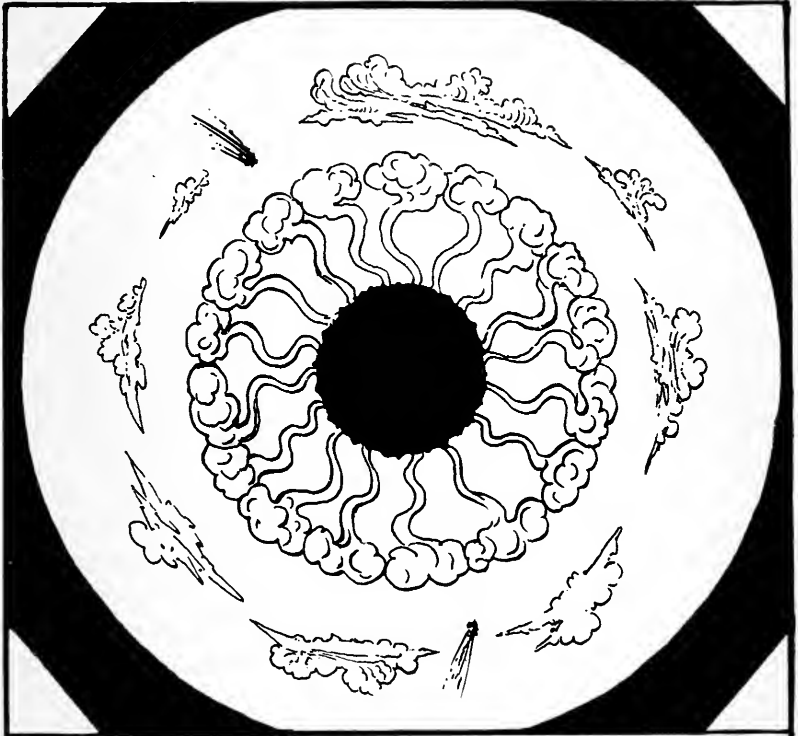


Fig 1.

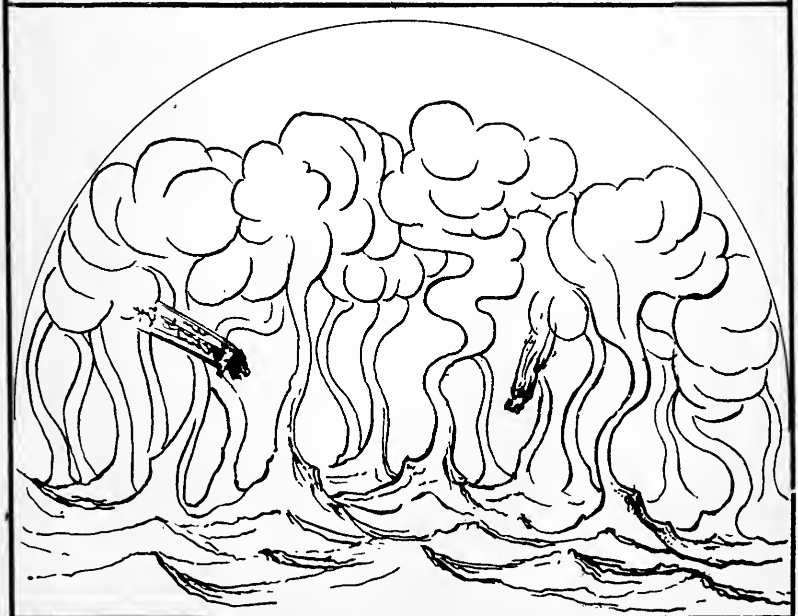


Fig 2.

We thus see that such a supply of matter as would be required to maintain the Sun's stock of heat, enormous as is his expenditure, is at hand.

(To be continued.)

Franklin Institute.

Proceedings of the Stated Monthly Meeting, November 18th, 1868.

The meeting was called to order with the Vice-President, Mr. Coleman Sellers, in the chair.

The minutes of the last meeting were read and approved.

The Actuary submitted the minutes of the Board of Managers, and reported the donations to the Library received at the stated meeting, held November, 11th inst.: from the Royal Astronomical Society, the Statistical Society, and the Chemical Society, London, England; la Société Industrielle, Mulhouse, France; der Osterreichen Ingenieur-veriens, Vienna, Austria; the Board of Public Works, Chicago, Illinois; the Louisville and Nashville Railroad Company, Louisville, Kentucky; Charles H. Hart, Esq., Professor J. Aitkin Meigs, and Hector Orr, Esq., Philadelphia.

The various Standing Committees reported their minutes.

The Secretary's report on Novelties in Science and Arts was then read.

Remarks were then made by Mr. Coleman Sellers, on the subject of the Huntoon Governor; by Prof. Robt. C. Rogers, on the Manufacture of Ruhmkorff Coils and Geissler Tubes, by M. Gaiffe; and on the New Theory of Puddling, by Mr. Robert Briggs, after this the meeting, on motion, adjourned.

Henry Morton, *Secretary.*

Bibliographical Notices.

London Chemical News. American reprint. W. A. Townsend & Adams, 434 Broome Street, New York.

In the number of this publication for December, which we have just received, we observe that a new feature has been introduced, in the shape of a supplement, under the editorial charge of Professor Charles A Seely, which contains notices of the current events in America relating to the progress of Chemistry and the Physical Sciences; also, reviews of new books, notices of the market, and movements of trade, bearing upon the same subjects. It would be unnecessary for us to comment upon the excellence and value of the *Chemical News*, which owes its existence and success to the ability and untiring energy of Dr. Crookes, on whom it reflects, by its well-merited success, no small credit. We are glad, for the credit of our country, that the American reprint is made with the approval of, and by arrangement with, the English editor, and cordially commend it to our readers as a journal of unequalled value in its own department, and in all respects worthy of their support.

Its columns are freely opened to writers on this side of the Atlantic, and their communications are treated with courtesy and respect. We observe the names of some of our most eminent chemists among its contributors; and it is, of course, the organ of all distinguished students in this branch abroad, and no follower of this youngest but most important of the sciences, should think his laboratory furnished without a set of this journal.

Physicians' Medical Compend and Pharmaceutical Formulæ. Compiled by Edward H. Hance. Published by Hance, Griffith & Co., Philadelphia.

This little pocket-book, of 214 pages, filled with recipes and memoranda such as would be required by the physician or apothecary when making up or dispensing medicines, as also a full list of poisons, with their antidotes and treatments, and a full index, seems to us to be a very useful addition to the well-stocked armory of weapons with which modern science fights disease. We are not in a position to speak of its accuracy, but, since it is a compilation, it is fair to presume that the best authorities have been accurately followed, which is all that could be asked in such a case.

A COMPARISON *of some of the Meteorological Phenomena of* NOVEMBER, 1868, *with those of* NOVEMBER, 1867, *and of the same month for* EIGHTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11¼′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	November, 1868.	November, 1867.	November, for 18 years.
Thermometer—Highest—degree	72·00°	69·00°	80·00°
" date	10th.	9th.	1st, '60.
Warmest day—mean	62·33	62·33	72·30
" " date	9th.	9th.	9th, '57.
Lowest—degree	33·00	24·00	16·60
" date	23d.	19th.	25th, '60.
Coldest day—mean	37·33	31·00	23·30
" " date	22d.	18th & 19th.	25th, '60.
Mean daily oscillation	12·10	14·47	13·20
" " range	5·37	6·37	5·68
Means at 7 A. M.	42·38	42·93	41·46
" 2 P. M.	50·30	51·33	50·42
" 9 P. M.	45·02	46·68	44·71
" for the month	45·90	46·98	45·53
Barometer—Highest—inches	30·418	30·342	30·661
" date	13th.	5th.	12th, '51.
Greatest mean daily pressure	30·369	30·325	30·520
" " " date	7th.	5th.	12th, '51.
Lowest—inches	29·355	29·250	29·080
" date	30th.	29th.	4th, '64.
Least mean daily pressure	29·426	29·593	29·150
" " " date	30th.	29th.	4th, '64.
Mean daily range	0·187	0·182	0·183
Means at 7 A. M	30·005	30·006	29·920
" 2 P. M	29·963	29·972	29·876
" 9 P. M	29·992	29·994	29·906
" for the month	29·987	29·991	29·901
Force of Vapor—Greatest—inches	0·486	0·548	0·832
" date	10th.	10th.	8th, '57.
Least—inches	·104	·100	·055
" date	12th.	18th.	25th, '57.
Means at 7 A. M	·212	·236	·223
" 2 P. M	·201	·256	·228
" 9 P. M	·217	·258	·233
" for the month	·210	·250	·228
Relative Humidity—Greatest—per cent	93·0	94·0	100·0
" date	18th.	10th.	Often.
Least—per cent	36·0	41·0	25·0
" date	12th.	16th.	7,'63 & 25,'66
Means at 7 A. M.	75·3	80·03	77·1
" 2 P. M.	52·6	63·0	58·4
" 9 P. M.	70·5	76·5	73·2
" for the month	66·1	73·3	69·6
Clouds—Number of clear days*	9·	8·	8·6
" cloudy days	21·	22·	21·4
Means of sky covered at 7 A. M	61·7 per ct	65·0 per ct	60·7 per ct
" " " 2 P. M	55·0	66·0	60·3
" " " 9 P. M	44·0	50·0	51·4
" " for the month	53·5	60·3	57·5
Rain—Amount—inches	4·53	2·540	3·613
No. of days on which rain fell	9·	10·	10·1
Prevailing Winds—Times in 1000	N70°21′W.258	S76°32′W.261	N74°32′W.256

* Sky one-third or less covered at the hours of observation

A COMPARISON *of some of the Meteorological Phenomena of the* AUTUMN *of* 1868, *with that of* 1867, *and of the same Season for* EIGHTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11¼′ W. from Greenwich. By PROF. J. A. KIRKPATRICK, A. M.

	Autumn. 1868.	Autumn. 1867.	Autumn. for 18 years.
Thermometer—Highest—degree	88·00°	86·00°	95·00°
" date	Sept. 12.	Sept. 19.	Sept. 12, '51.
Warmest day—mean	82·83	77·83	85·20
" " date	Sept. 12.	Sept. 6.	Sept. 6, '54.
Lowest—degree	33·00	24·00	16·00
" date	Nov. 23.	Nov. 19.	Nov. 25, '60.
Coldest day—mean	37·33	31·00	23·30
" " date	Nov. 22	Nov. 18 & 19.	Nov. 25, '60.
Mean daily oscillation	12·09	14·79	14·65
" " range	5·40	5·67	5·40
Means at 7 A. M.	51·80	52·25	51·98
" 2 P. M.	60·24	62·83	62·50
" 9 P. M.	55·10	56·42	55·67
" for the Summer	55·71	57·17	56·72
Barometer—Highest—inches	30·552	30·466	30·661
" date	Oct. 30.	Sept. 24.	Nov. 12, '51.
Greatest mean daily pressure	30·541	30·456	30·541
" " " date	Oct. 30.	Oct. 24.	Oct. 30, '68.
Lowest—inches	29·355	29·250	29·012
" date	Nov. 30.	Nov. 29.	Oct. 26, '57.
Least mean daily pressure	29·426	29·593	29·059
" " " date	Nov. 30.	Nov. 29.	Oct. 26, '57.
Mean daily range	0·167	0·164	0·151
Means at 7 A. M.	30·065	30 048	29·937
" 2 P. M.	30·022	30·009	29·894
" 9 P. M.	30·045	30·024	29·921
" for the Summer	30·044	30·027	29·917
Force of Vapor—Greatest—inches	0·846	0·812	0·991
" date	Sept. 11.	Sept 17.	Sept. 6, '54.
Least—inches	·094	0·100	·055
" date	Oct. 17.	Nov. 18.	Nov. 25, '57.
Means at 7 A. M.	·322	·345	·339
" 2 P. M	·336	·375	·356
" 9 P. M	·342	·379	·359
" for the Summer	·333	·366	·351
Relative Humidity—Greatest—per cent.	97·0	100·0	100·0
" date	Sept. 23.	Oct. 29.	Often.
Least—per cent.	28·0	28·0	23·0
" date	Oct. 17.	Sept. 30.	Oct. 21, '59.
Means at 7 A. M.	75·6	80·6	77·7
" 2 P. M.	57·6	59·9	57·2
" 9 P. M.	71·9	76·4	73·6
" for the Summer	68·4	72·3	69·5
Clouds—Number of clear days*	21·	34·	28·8
" cloudy days	70·	60·	62·2
Means of sky covered at 7 A. M.	66·6	62·0 p. c.	59 0 p. c.
" " " 2 P. M.	62·2	53·2	56·3
" " " 9 P. M.	52·5	44·0	43·5
" " " for the Summer	60·4	53·1	52·9
Rain—Amount—inches	15·38	8·410	11·092
No. of days on which rain fell	34·	24·	27·4
Prevailing Winds—Times in 1000	N56°19′W.162	N86°12′W.209	N77°23′W.217

* Sky one-third or less covered at the hours of observation.

JOURNAL

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Vol. LVII.] FEBRUARY, 1869. [No. 2

Editorial.

ITEMS AND NOVELTIES.

The Dubuque Bridge—Official Report of the Test.—We are indebted for a copy of the following report to Walter Katte Esq., Engineer of the Keystone Bridge Company:

Having been invited by the Chief Engineer and other officers of the Dunleith and Dubuque Bridge Company to witness the test examination of their Railroad Bridge, just completed, over the Mississippi river, at Dubuque, we attended for that purpose, on the morning of Tuesday, December 29.

Col. R. B. Mason, Chief Engineer, assisted by J. E. Ainsworth, resident Engineer, had previously arranged five first-class locomotives, with the tenders, supplied with their full complement of fuel and water, making a gross weight, as estimated by Mr. J. C.

Jacobs, Superintendent, of about 246 tons. This train was run over the whole length of the bridge both ways, and was also allowed to rest some time upon single spans, being a little more than the length of the longer spans, which are 250 feet, and, as nearly as may be, one ton per lineal foot of the span.

With the above load the greatest deflection in the middle of the span was 0·138 of a foot, or about one inch and twenty-one thirty-seconds. On removing the load the span resumed its height at the centre, within one thirty-second of an inch.

The Draw Span, on its western half, which is 180 feet long from the pivot, was subjected to as much of the weight as it would contain on that length, or about one ton per foot lineal, and showed a deflection of 0·066, or about three-fourths of an inch. On removing the load the span resumed its height within 0·014 of a foot, or about one-sixth of an inch.

Applying a similar test of about one ton to the foot lineal of the span of 225 feet on the sixth span, the deflection in the middle of the span proved to be 0·128 of a foot, or one seventeen thirty seconds inches. On removing the load it assumed its original height, within 0·005 of a foot, or one-sixteenth of an inch. A similar test of the seventh span showed a deflection of 0·127 of a foot, or one and one-half inches. On removing the load it rose to its original height, within one hundreth of a foot—less than one-eighth of an inch. The span was then tested with an engine and one passenger car, and showed a deflection of 0·06 of a foot, or three-fourths of an inch. We also passed over the bridge with an engine drawing its usual passenger train, making a general examination of the entire structure.

All our examinations have satisfied us that the bridge is an excellent and safe structure, admirably adapted to the purpose for which it is designed: namely, to connect the railroads of Iowa and the Union Pacific Railroad with the railroads running through Illinois to Chicago and the eastern cities.

We learn that this bridge has been erected by a Company composed chiefly of residents of the city of Dubuque. The following are the names of the officers :—Hon. W. B. Allison, President; Wm. E. Massey, Secretary; Platt Smith, H. E. Stout, J. Tucker, and R. B. Mason, Directors; R. B. Mason, Chief Engineer; J. E. Ainsworth, Resident Engineer.

The sub-structure was in the hands of Reynolds, Salpaugh & Co. of Rock Island, who have fulfilled their contract in a very short

time, entirely to the satisfaction of the Company, and deserve great credit for their energy and skill.

The stone for the piers was brought by rail from Joliet and Lamont, Illinois.

The iron superstructure was built at Pittsburgh, Pa., by the Keystone Bridge Company, and put up by them in about eight months, under the supervision of Walter Katte, Esq., their Engineer.

It presents altogether one of the most remarkable engineering performances of the age. Eleven months ago the iron that now constitutes one of the finest bridge superstructures in the country existed only in the shape of the natural ore, or pig iron. Since that time it has been manipulated and erected as it now spans the majestic Mississippi.

The bridge is built from the designs of J. H. Linville, Civil Engineer, President of the Keystone Bridge Building Company. It consists of seven spans, two of 250 feet, four of 225 feet, and one of 360 feet, resting on the pivot pier, leaving the opening 160 feet wide on each side of the pier for river navigation. The total length of the bridge is 1760 feet, or exactly one-third of a mile. The truss is 28 feet high and 16 feet wide in the clear. The piers are of stone, founded on piles—about 180 piles to each pier, driven to an average depth of 27½ feet, cut off at about 13 feet below low water.

The eastern approach of the bridge is made by a tunnel, cut through tbe rock of the bluff, 835 feet long, 22 feet high, and 18 feet wide, the rock being solid and requiring no arching. The approach on the west side is made on strongly-built trestle-work, 3200 feet long. The distance from the depot, on the Iowa side, to the eastern end of the bridge, laid by the Bridge Company, is 6902 feet. There are 4772 cubic yards of masonry in the piers and abutments. In the superstructure there are 2,405,000 pounds of wrought iron, and 462,800 pounds of cast iron.

The lower chord of the superstructure is 31½ feet above low water, and ten feet above the highest known flood—the range of the water at this point being 21 feet.

In concluding this hastily prepared report, we beg leave to tender our acknowledgments to the officers of the Illinois Central and Northwestern Railroad Companies, for the courtesies extended by them, and to the officers of the Bridge Company, and the citi-

zens of Dubuque, for their numerous kind attentions during our visit to their beautiful city.

W. MILNOR ROBERTS,
Chief Engineer St. Louis and Illinois Bridge.
E. B. TALCOTT,
Civil Engineer.
E. H. JOHNSON,
Chief Engineer C. R. I. and P. Railroad.
JOHN E. BLUNT,
Engineer Galena Div. C. and N. W. Railway.
A. S. VAN MEENAN,
Engineer Wisconsin Div. C. and N. W. Railway.
GEO. W. MORGAN,
Engineer W. Div. P. F. W. and C. Railway.

Concrete Bridge.—The tests applied to the experimental bridge of concrete, set in cement, erected over that branch of the Metropolitan District Railway which forms one of the junctions between the circular line and the West London Extension, prove conclusively the reliable character of concrete exposed to compressive strains. The structure experimented upon spans the open cutting between Gloucester-road Station and Earle's Court road. It is a flat arch of 75 feet span and 7 feet 6 inches rise in the centre, where the concrete is 3 feet 6 inches in thickness, increasing towards the haunches, which abut upon the concrete skewbacks. The material of which the bridge is made is formed of gravel and Portland cement, blended in the proportions of six to one, carefully laid in mass upon close boarding set upon the centering, and enclosed at the sides.

In testing the bridge, rails were laid upon sleepers over the arch, which brought a load of $\frac{2}{75}$ths of a ton per foot run upon the structure. Seven trucks, weighing, together with their loads, 49 tons, were formed into a train, having a wheel base of 57 feet; hence the rolling load amounted to $\frac{49}{57}$ths of a ton per foot run. The deflection produced by the passage to and fro of this train four times, was noted upon a standard, cemented to the side of the arch, at a distance of one-third the span from the abutments. When one side of the bridge was loaded, the extreme rise of the branch on the opposite side was about $\frac{1}{16}$th of an inch, which was produced by a maximum strain of 10 tons 14 cwt. per square foot.

At a subsequent trial, a mass of gravel, 10 feet wide and 3 feet thick at the crown, and 6 feet deep at the haunches, was laid over

the bridge, and upon this, ballast was placed the permanent way. After an interval of a few days, the trucks, loaded as before, were passed over the bridge, at first in pairs, and finally all together. In this test the strain upon the concrete was as follows:

The weight of the arch, as before	7 tons 17 cwt.
170 tons of ballast	4 tons 8 cwt.
Strain per square foot from dead load	12 tons 5 cwt.
Strain per square foot from passing load	2 tons 17 cwt.
Total strain per foot	15 tons 2 cwt.

After repeated transit, the load was left upon the bridge all night, and the arch, upon examination, showed no signs of failure or distress under the severe strains to which it had been exposed.

From these trials it is fair to assume, that a thoroughly well-constructed arch of concrete is absolutely stronger than a similar one of brick; but in practice the danger arises that it would be difficult to ensure so high a quality of concrete as that employed in the present instance, and the proper supervision of the contractor's work by the engineer would be almost impossible in structures of this material, whilst the inspection of brick work is an easy matter.

The utter uselessness of inferior concrete was shown by the failure of the bridge which was previously erected on the site of the present one, which yielded under its own load when the centres were struck.

The Cincinnati and Newport Bridge.—All preliminary arrangements and work has now been begun upon this bridge, which is to connect Butler Street, in Cincinnati, with Saratoga Street, in Newport, Kentucky.

The stone work of all the piers is to be of the best limestone, up to the line of high water, and freestone above that, excepting the two piers of the middle, or long span, which will be entirely limestone. Much of the stone for the piers has already been quarried; George A. Smith, of Cincinnati, has the contract for the stone work.

The bridge proper will be of the best wrought iron, in lower and upper chords, up-rights, braces, &c. No timber will be used save in the flooring. The Keystone Bridge Company has the contract for the bridge proper, which will be constructed after the popular and very safe patent of Linville & Piper, now in quite general use in

this country. It was upon this plan that the Steubenville bridge was constructed.

The floor of the main span, on which the train is seen, will be about 100 feet above low water. This span is planned at a length of 420 feet; the one next south is 240 feet, and the others as near 200 feet each as the division of distance will admit. There will be seven spans in all, with the eight piers. Beyond the front streets of both Newport and Cincinnati, the grade to the cities will increase, that of the wagon tracks being much sharper than the longer and easier one of the railroad.

The bridge will be forty-one or two feet in width, with thirteen feet in the middle for trains, one way on either side for cattle and vehicles, and on the outside of these still, the passages for foot-passengers.

This bridge will be built by the Newport and Cincinnati Bridge Company, organized with a capital of $1,200,000, and having the following as its Board of Directors and officers; Alfred Gaither, President; Albert S. Berry, Vice-President; Charles H. Kilgour, Secretary and Treasurer; M. J. King, William Ringo, W. H. Clement and T. G. Gaylord.

They contemplate having a train cross this bridge by the 1st of December, 1869. J. H. Linville, of Philadelphia, is the chief engineer and supervising architect. Mr. John C. Wilson is the resident engineer, with an office at the north-west corner of Pearl and Butler Streets, Cincinnati.

Passage through the Suez Canal.—The Rob Roy, an English merchant vessel, recently passed through the Suez Canal, and the captain writes to the *London Times* the following account of the present condition of this great undertaking, after thirteen years have been spent in its construction:

"The canal, as designed, is about a hundred miles long. Of this length, about half is sufficiently advanced for the sea water to reach fifty miles—that is, into the middle of the isthmus. It is finished to its full breadth, which is a hundred yards, or the width of a considerable river, but not to the intended depth of twenty-six feet. The remaining fifty miles not yet penetrated by the sea water, are in various states of progress; parts are excavated, parts are under water, parts will have to be laid under water which is to be supplied from a great lake not yet filled, while a good many miles have to wait for large blasting operations. To English ears it must sound

promising that a good deal of clay has to be cut through; for nothing can be dealt with so successfully in this country, as that material. The completion of the southern half of the canal would look like a very long work, but for the fact of the immense subsidiary works being completed, and a vast mass of appliances being on the spot. The service canal, from the Nile to the mid point of the salt water canal, and branching thence to either extremity, is an immense work, not less than a hundred and fifty miles long, and in full use for the supply of fresh water for navigation, and for otherwise assisting the work to be done. The port at the Mediterranean end is an immense work, already available. The sea channel at the Suez end has difficulties, but only such as engineers are familiar with. Forty enormous and costly dredging machines are at work on different parts of the canal—chiefly, we conclude, the northern half—discharging mountains of mud, sand and clay over the banks or into barges. The rate of expenditure is put at £200,000 per month, or two and a half millions a year. Our informant calculates that a driving wind, after blowing a month together, will send into the canal, when finished, five hundred tons of sand a day, or fifteen thousand tons a month. This, however, is no more than a single dredging machine would be able to keep down at a certain moderate cost in coal. The difficulty of keeping up the banks of the canal, exposed as they will be to the wash of steamers, and to a surface often agitated by the wind, is a more serious matter, but one which does not enter into the present question. Upon the whole, it does seem a moral certainty that, at least in two or three years—for one year seems out of the question—this great undertaking, worthy of a heroic age, will be brought to what we may fairly call an actual completion. In the course of the year 1871, we may probably see the sea water of one ocean flowing into the other."

Hoosac Tunnel.—It is announced that the Hoosac Tunnel contract has been disposed of, so far as abstract propositions are concerned, and that the executive council are now at work upon the details. Messrs. Shanly Brothers, of Canada, are the successful parties, and their bid is stated to be $4,750,000. The terms of security have been so far modified as to allow the parties contracting to finish work on the tunnel to the amount of $500,000 before drawing from the Treasury in lieu of furnishing the like amount in bonds.

The Denver Pacific Railroad it is stated has been graded the

entire distance, 105 miles, from Denver to the connection with the Union Pacific. The entire work is paid for. The track it is asserted, will be laid early in the spring.

The Tunnel under Chicago River, at Washington Street, was formally opened to the public, January 1st. The event was celebrated by the passing through it of a procession composed of the Mayor, Common Council, Fire Companies and distinguished guests. The authorities consider it such a success that they have advertised for proposals for two more at different points of the river.

Chromium Steel.—It has long been known that an alloy composed of sixty parts of chromium and forty parts of iron is so hard as to scratch glass like a diamond, and such an alloy may be formed by heating oxide of chromium in a blast-furnace, with metallic iron. Experiments are now being made to produce a species of steel, suitable for rails and other purposes, by adding chrome ore and manganese to the iron in the puddling-furnace; and the results are said to be promising, though not conclusive.

Photography and Gunnery.—During the late experiments at Fortress Monroe, photographs were taken of the target from an adjacent bomb-proof, so as to record the exact amount of damage effected by each shot.

Photographing a Tunnel.—We have lately seen a photograph taken by sunlight of the interior of the tunnel which penetrates the summit of the Nevada for the distance of 1659 feet, at an elevation of 7,042 feet above the sea level, the greatest height to which a locomotive has yet attained. The success of this picture is due to the position of this tunnel, through which like that near Bore, on the Great Western Railway, the sun shines for a few days each year. Taking advantage of this circumstance, and also using mirrors by which light was thrown successively upon various points, this picture was produced with an exposure of fifteen minutes, and shows the distant heading with perfect distinctness as well as the long intermediate cavern.

Magnesia Blocks for the Lime Light.—Through the kindness of Mr. James Swain, one of our own citizens, now and for a long time resident in Paris, and from his scientific taste and acquirement thoroughly *en rapport* with all the new things, we have received a number of those blocks of compressed magnesia, about which so much has been said of late in the foreign journals. They are square prisms of about three-quarters inch base, and five-

eighth inch in height, of remarkably even texture, and notable density. Notwithstanding all that has been said in their praise, they do not prove on trial to be by any means equal in powers of resistance (when submitted to the oxyhydrogen flame) to the average quality of lime. With a pressure of about three inches of water at the jet, they are rapidly eaten away, and moreover split by action of the heat. It is just possible that these specimens have suffered some deterioration in their transit across the ocean, though considering their method of packing, and the properties of the material, this is hardly probable. Their one undoubted advantage seems to be their security from injury on exposure to the air. In this respect, having a marked advantage over lime. Except, however, where exceedingly low pressures are employed, their rapid destruction before the jet, more than compensates for the other advantage.

Cement for Stone.—It is stated that an admirable cement is obtained by mixing infusorial silica, such as constitutes the diatoms, for instance tripoli, with the following substances. The infusorial silica is mixed in about equal proportions with oxide of lead; about half a part of freshly slaked lime is then added, and the whole is made into a paste, with boiled linseed oil. This is especially recommended for securing iron-work in marble and other stone.

Pacific Railway.—According to the New York advices, regarding the progress of the Pacific Railway, the work will be completed by next July, and at that time there will be a continuous line of rail from New York to San Francisco, about 3,300 miles, the longest in the world. A European passenger will then, it is calculated, be able to reach Shanghai, China, *via* New York, in thirty-eight days, while the shortest time in which the trip can now be made, going eastward, is sixty days. At a recent meeting at Chicago, an examination would show that goods could be gotten from Asia to Chicago at first hands, at a much less cost than they can be bought in New York, which is 955 miles by rail, and 1,500 miles or so by lake, canal, and the river Hudson to the eastward.

The Heaton Steel.—Most of the English journals have for some time past contained lengthy arguments and discussions on Mr. Heaton's process for converting iron into steel, by running the melted iron into a vessel at the bottom of which has been previously placed, under a perforated iron plate, a quantity of nitrate of soda, the oxygen of which, liberated by the heat, and rising through

the molten mass, burns out its impurities. After a review of the subject, it appears to us that the drawbacks to the proeess are, the uncertainty of the product and its want of purity.

Suez Canal.—Between Shalouf and Suez, a distance of only 20 miles, about 20,000 men are at work. The workmen are organized in the manner described in this *Journal*, Vol. LV., 380 p.and the work is being pushed with great vigor, steam dredges, asses, mules, men, and camels, all contributing towards their completion. The contractor, M. Levalley, is under a penalty of £8,000 a month, to complete the works by the 1st of October next.

An Explosive Dye.—The artificial saffron, invented by Mr. Millonsweg, of Poblitz, has of late been found to be as easily exploded as gunpowder, though possessing forty per cent. less of projectile force.

Moss Rubber Inking Roller.—The editor of the *Mechanics' Magazine*, London, speaks very highly from his own experience of the above mentioned article, which is invented by Mr. Stephen Moulton, of Bradford-on-Avon, and prepared in the following manner. Vulcanized india rubber is reduced to a powder, then placed in a mould, and subjected to a second vulcanizing heat, which converts it into a mossy substance. This core is now covered with skin of rubber, which is then vulcanized, and the roller is then ready for use.

Detonating Gun Cotton.—Some late experiments by Professor Abel and Mr. Brown, seem to demonstrate that compressed guncotton if exploded by a detonating fuse, possesses all the power and consequent advantage of nitro-glycerine, so as to effect blasting without tamping, and to shatter heavy iron plates when fired loosely upon them.

Gradually Adjustable Stop, or diaphragm for lenses. By J. Zentmayer.

At the last meeting of the Institute, there was exhibited this exceedingly ingenious arrangement, which is shown in the accompanying cuts, which are taken from photographs, Fig. 1 showing the apparatus with its largest, and Fig. 2 with its smallest, opening. To obtain a circular diaphragm which, like the eye, should expand and contract gradually by a continuous change, and yet be made of rigid and unchangeable material, might seem, at first sight, to be an impossibility; but, after all, when the result is accomplished, as

in this apparatus, we are surprised no less by the simplicity than by the ingenuity of the means employed.

The wood-cuts almost explain the apparatus of themselves; but we may say in addition, that it consists of two cylinders or rollers with parallel axes and surfaces in contact, having similar conical grooves on their surfaces, and fine teeth cut at one end of each, which, gearing together, cause them to rotate in unison.

Fig. 1.

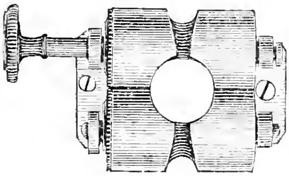
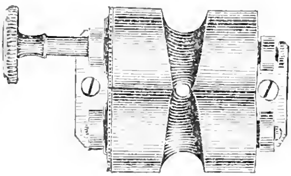


Fig. 2.



There is, theoretically, an objection to a diaphragm of this construction, from the fact that its opening will not always be in the same plane—that is, the smallest cross-section of the space between the rollers will not always be equidistant from a plane at right angles to the line of sight and passing through the axes of the rollers. With the larger opening, this smallest section will be nearest to, and with the smaller, further from, such a plane.

In practice, however, this difference is so small as to be entirely unimportant, and may even, in some cases, be turned theoretically to advantage.

Secondary Batteries.—In a late number of the *Annales de Chemie* we find an elaborate article, of some twenty pages, by M. Gaston Planté, on the above subject. By omitting the historical discussion, and such minute details of experiments as could only be of use to one who wished to make a literal copy of each step, we think that the valuable results embodied in this essay may be expressed in a small space.

If two leaden plates, plunged in diluted sulphuric acid, are made terminals of a small galvanic battery, the one connected with the negative pole, will have its surface rendered perfectly clean and metallic by the reducing action of the hydrogen there liberated, while the opposite plate, connected with the positive pole, will, on the other hand, be rapidly per-oxidized by the nascent oxygen set free at that point. If, then, the battery being disconnected, these two plates are put into communication, they will be in condition to

develop a galvanic current opposite in its direction to that which was before passed through them by the battery. Thus the clean plate or former negative pole will tend to oxidize, decomposing the water of the pile and liberating hydrogen, while the peroxidized plate will tend to absorb this hydrogen and suffer reduction. These actions are precisely such as are needed to produce a galvanic current, which, experience proves to be of a very intense character though short duration. Availing ourselves of this fact, it becomes possible to construct what, from its practical result, may be called a *condenser* for *dynamic electricity*: thus we may employ a small battery, during a notable time, to bring a number of connected lead plates into the condition above described, and then use for a few moments, the intense current they are able to generate, thus obtaining a concentrated and intense momentary effect from a feeble but continued supply of force, not as in the Leyden jar or other condenser of statical electricity, by an actual accumulation and storing up of the feeble force, but by the indirect method and through the intervention of the chemical reactions above described.

M. Planté found it convenient, in his experiments, to employ troughs of gutta percha, each containing 20 plates of lead 10 inches square and $\frac{1}{50}$th of an inch thick, connected as a single pair of elements—that is, the odd numbers of plates united together by a copper strip at one side of the trough, and the even numbers similarly joined on the other side, the trough being filled with a mixture of ten parts of water to one of sulphuric acid. Two such batteries, connected as a single pair, could be fully charged in a few minutes by three small Bunsen cells with carbons 3 inches high (connected for tension), and would then suffice to bring to incandescence thick pieces of platinum wire to fuse short wires of iron, steel, and other metals, to dephlagrate steel needles, and this for a sufficiently long time to burn up a steel needle four to five inches long. By careful measurement, it was, indeed, proved that the electro-motive force of each couple in this secondary battery—during its time of maximum action—was equal to ·2362, that of the Bunsen element mentioned above, being 0·164; in other words, the electro-motive force of these elements was, to the Bunsen, as 1·44 to 1. With regard to resistances, these were nearly equal, being as 27 to 23. By placing each pair of plates in a separate trough, and employing an extensive but simple commutator by which they

could be connected in quantity to receive the current of the exciting battery, and in series to obtain their own current, effects of very great intensity were secured; thus a platinum wire 6 feet long and 100th of an inch in diameter was heated to incandescence; a similar steel wire was instantly fused, the combustion of mercury and the electric light was readily produced, and, in a word, all the effects which could be obtained continuously from a pile from 55 to 60 Bunsen elements of equal surface were secured, intermittently, from this secondary pile. By another arrangement of the commutator a constant series of successive currents was obtained.

This had been accomplished before by M. Thomsen, of Copenhagen, who, in 1865, arranged an apparatus on this general principle for operating a telegraph line.

While the subject of secondary batteries was receiving such development abroad as is indicated by the above abstract, the same branch of electrical research was pursued in this country by Dr. George Percival, and has been brought by him to a point of decidedly higher efficiency as regards practical results and theoretical perfection.

Considering that where both electrodes of the secondary pile were of lead, a loss of effect was experienced, by reason of the low electro-motive force of this metal, he proposed to make the positive electrode of the secondary series, of some more active metal, such as zinc, and then to use as electrolyte, not sulphuric acid and water, but a neutral solution of the positive metal. Thus: with zinc as positive element, a solution of sulphate of zinc; with copper, sulphate of copper; or with iron, sulphate of iron. By this means, by the action of the charging battery, not only is peroxide of lead deposited on the negative electrode of the secondary (which is, of course, connected with the positive pole of the charging battery, and is the positive electrode with reference to the charging current), but there is also a deposit of finely divided zinc on the positive element, and a liberation of sulphuric acid in the solution. By this means, a much more energetic chemical and electrical action is developed when the charging current ceases, than if both electrodes were of lead, and the positive element therefore of this feebly active material.

It is found best to amalgamate the zinc plates of this battery, and as this renders them brittle, it is necessary that this should be done after all their connections are made, and, in fact, the entire

arrangement ready for use. To accomplish this, the series of plates is connected wtth another battery, so that the zinc plates form the negative electrode, and plunged in a solution of 3 ounces of bisulphate of mercury and 8 ounces of sulphate of ammonia in a quart or so of water.

When one of these secondary batteries is charged, it may be removed from the solution and dried, and will then retain its capacity for developing a powerful current for many months.

Fire-Alarm, by M. Diou. At the last meeting of the Institute there was presented by Mr. J. Demorat, for exhibition, an improved fire detector, manufactured by the American Fire Detector Company, 725 Broadway, New York.

This apparatus consists of two parts, one of which is placed in the location where the presence of fire is to be detected, and the other where the alarm is to be given. These are connected by wire, in the manner of ordinary bells, except that the wire is tightly stretched in its normal condition.

The first instrument consists, essentially, of a catch, (holding one end of the wire) controlled by a copper helix, whose expansion will liberate the catch, and thus slack the wire. The other instrument consists of an alarm bell, operated by clockwork, which goes into action as soon as the wire is slacked.

By changing the tension of the copper spring, the instrument may be set to go off at any temperature, indicated by a dial and pointer attached to the regulating screw.

When exhibited to the meeting, the instrument was started by holding it, for a moment, over a gas flame, and by the mere warmth of the breath.

Lining for Fire-proofs.—There was exhibited, at the last meeting of the Institute, a new material, which, by its remarkable power of non-conduction, presents especial advantages. This was devised and patented by Mr. W. Alford. It consists of a rough *papier maché*, made of old wall paper, by moistening and compressing it. Its power of resistance to fire was illustrated by a specimen exhibited on this occasion, which had been exposed, as a lining to an iron box, with a wooden one in the centre, to the heat of a brightly burning anthracite fire, for the space of an hour. The material was charred on the outer surface, to the depth of about one-fourth of an inch, while all the rest, and the box, of course, within, was perfectly intact. This substance has been favorably

reported upon by many of our safe-builders, and seems to be an admirable invention. A melancholy interest is attached to the subject, from the fact that the day on which these specimens were left at the Institute by Mr. Alford is the last on which he has been seen by his family or friends. No sort of clue has been found to this most mysterious disappearance, which would seem to indicate some fatal accident.

A new Spectrum Discovery.—Nearly two years ago, Mr. Norman Lockyer—the editor and translator of "Guillemin's La Ciel," ("The Heavens," in its English form,) author of an excellent little book entitled Elementary Lessons in Astronomy, which contains some of the best engravings of the sun and planets which have been published, and a young astronomer of high reputation—announced a plan by which he proposed to examine the luminous rose colored prominences seen around the sun during total eclipse, by aid of the spectroscope, on ordinary occasions, and without the necessity of an opaque body to cut off the sun's general light.

It was founded upon the following considerations: If these prominences were composed of intensely heated gas, they would produce bright lines in the spectroscope. Now, these lines, being each purely monochromatic, of a fixed refrangibility, or non-composite nature, would not be dispersed or spread out by any number of refractions, hence their intensity of light would remain but little impaired after a great many refractions, or passage through a large number of prisms. The general white light of the sun, however, would, on the contrary, be continually dispersed in proportion to the extent of its refraction, and might thus be finally diffused and outspread by this means, until its brightness no longer masked that of the relatively faint and imperceptible lines due to a heated gas.

With this view, he constructed an instrument, and made observations of the sun, hoping to see a gaseous spectrum superposed upon the solar one, or continuing it, along the edge of the sun's disk. In this, however, he failed, and was thus led to suppose that the red clouds or flames, might not be gaseous after all.

The observations made during the late eclipse, however, proved that these prominences *were* gaseous, and developed bright lines. He therefore justly concluded that his ill success was chargeable to his instrument, and by improvements in this, he was enabled to demonstrate at once the truth of his theory, and some most im-

portant facts in regard to the constitution of the sun. This successful observation was made on the 20th of October, with an instrument constructed by Mr. Browning. The slit of the spectroscope being turned to the edge of the sun's disk, a solar spectrum, due to the light from the disk, is seen, and upon this another, of bright lines, due to a luminous gas. If a luminous prominence of some height is situated at this point, the gaseous spectrum is of considerable height, and by carrying the spectroscope around the edge of the disk, and, noting the height of the gaseous spectrum at different points, the shape of the luminous prominences may be mapped out.

Mr. Lockyer, however, finds that a gaseous spectrum, of a certain height, is present at all points, and hence concludes that an envelope of luminous gas surrounds the sun to a distance of about five thousand miles, while the luminous prominences are only accumulations of this same gaseous matter, reaching sometimes a height of seventy thousand miles.

On the 18th of August, Mr. Jansen, of the French observing party, sent to India to make record of the total eclipse; made a similar discovery as to the possibility of observing the prominences without an eclipse. While, therefore, Mr. Lockyer was the first to propose the method, Mr. Jansen was the first to make the actual discovery, though Mr. Lockyer did the same thing, independently, afterwards, before any account from Mr. Jansen had reached Europe. The honor of the discovery might then be fairly divided between these two physicists.

Spectrum observations during the late *eclipse*, of which a full account, with a colored plate, appears in the last number of the London *Quarterly Journal of Science*, seem to indicate that the prominences or flames consist of intensely heated hydrogen, and of some body giving a yellow line, very near to that of sodium. The lines on which the various observers seem to agree best are a red one at or very near a yellow at or near D, a blue at F, and a violet at G.

Civil and Mechanical Engineering.

BELTING FACTS AND FIGURES NO. II.

By J. H. Cooper.

(Continued from page 43.)

Deductions and conclusions drawn from Table.

"Pulleys covered with leather, with grain-side of band to pulley, will sustain 50 *per cent. more resistance* than without the pulley being covered. The per cent. of resistance of the bands on the different pulleys is nearly as follows; and this per cent. will indicate the relative working value of each pulley respectively:

Iron pulley covered with leather	36	per cent.
" polished	24	"
" rough turned	15	"
Wood pulley, polished mahogany	25	"
	100	"

"Full 6 per cent. should be added to the polished iron pulley, to make allowance for the difference between commencing to slip and its sliding; thus making polished pulley 30 per cent., or next in value to leather.

"The relative or comparative working per cent. of the different bands, as indicated by the Table, is nearly as follows:

Leather, grain-side to pulley	34	per cent.
" flesh " "	23	"
Rubber	21	"
Gutta Percha	14	"
Canvas	11	"
	100	

"Thus leather belts, grain-side to pulley, will drive 34 per cent. more than flesh-side to pulley; 48 per cent. more than rubber; 121 per cent. more than gutta percha; 180 per cent. more than canvas; consequently, the very best arrangement for belting is to use it with grain-side to pulley, and have the pulley covered with leather. This is best in all cases. The next best pulley is polished iron, especially for quick motions. Polished wood next, and rough iron least in value.

"Leather, used with grain-side to pulley, will not only do more work, but last longer than if used with flesh to same. The fibre of the grain-side is more compact and fixed than that of the flesh, and more of its surface is constantly brought in contact or impinges on the particles of the pulley. The two surfaces, that of the band and that of the pulley, should be made as smooth as possible: the more so the greater the contact surface, and the more the particles of each impinge on the other. The smoother the two surfaces, the less air will pass under the band, and between it and the pulley—the air preventing the contact of band with pulley—the greater this contact, the more machinery will the band drive. The more uneven the surface of band and pulley, the more strain will be necessary to prevent bands from slipping. What is lost by want of contact, must be made up by extra strain on the band, in order to make it drive the machinery required—oftentimes, if the band is laced, causing the lacings to break, the holes to tear out, or fastenings of whatever kinds to give way.

This want of contact is noticeable on most of new bands used with flesh-side to pulley, and is distinctly marked by dark impressions on the band where it comes in contact with the pulley. Oftentimes not half of the surface will be found to have come in contact, and until it is worn smooth, or filled in with other substances, the full extent of the power of the band is not obtained.

* * * * * * * * *

"Bands used with grain-side to the pulley will never crack; as the strain, in passing it, is thrown on the flesh-side, which is not liable to crack or break, the grain not being strained any more than other portions of the band.

Rule for determining the width of belts.

$$W = \frac{5334\ HP}{V\ C}$$

In which W = width of belt in inches.
HP = horse-power transmitted.
V = velocity of belt per minute in feet.
C = part of circumference of smaller pulley in contact with belt, in feet.

Patent Angular Belting.

"This invention consists of a novel belt of a trapezoidal form, to be used in connection with a V-shaped or angular grooved pulley,

for driving all kinds of machinery where belting is required to transmit power.

"The angular belt is a great improvement on the round, square, or flat belt; a much greater surface of the belt is brought in contact with the pulley than with any other kind. It will wedge itself into the groove and resist any slipping action during the rotation of the pulley, so that the greater the strain put on one side of the belt the tighter will it be held in the groove; not being liable to slip on the pulley, it may be used very loose, causing less friction, consequently requiring less power to drive machinery, and giving it greater certainty and regularity of motion. As the angular band is not liable to slip, it can be used with greater economy and certainty than either the round or flat belting; its power is limited only by its strength; made of the same materials and the same width of belt, this form has more strength than any other.

"This belt has been used with perfect success, when other bands have failed entirely to impart motion to machinery. They are made without a joint in their length; and when the width requires more than one thickness of leather, the belt is connected, then rivetted or screwed, so that the fastening will not come in contact with the pulley."

Reasons why Rubber, Gutta Percha, and Canvas are the dearest articles to use for Bands.

"Under the same circumstances, and on the same machines, these bands will not last or wear one-fourth as long as leather. When once they begin to give out, it is next to impossible to repair them.

"Wide bands cannot be used for or cut up into narrow ones, as leather can be.

"Leather belts may be used over and over again, and, when of no further value for belts, can be sold for other purposes.

"A rubber band, costing hundreds of dollars, may be spoiled in a few moments, by the lacing giving out, and the band being run off into the gearing, or by being caught in any manner so as to damage the edge, or by stoppage of either the driving or driven pulley. A few moments of quick motion or friction will roll off the gum from the canvas in such quantities as to spoil the band.

"Leather belts may be torn or damaged, yet are easily repaired.

"Should a rubber or gum belt begin to tear by being caught in

the machinery, if the rent strikes the seam, it is most certain to follow it, even the entire length, if the machinery is not stopped. It would be impossible to tear leather in like manner.

"Oil, in contact with rubber belting, will soften the gum. * * *

"Rubber, gutta percha, and canvas belts will contine to stretch as long as in use, rendering it necessary to shorten them continually.

"During freezing weather, if moisture or water finds its way into the seams, or between the different layers of canvas composing these bands, and becomes frozen, the layers are torn apart, and the band is spoiled: or if a pulley becomes frosty, the parts of band in contact with it will be torn off from the canvas and left on the pulley.

"Gum belts will not answer for 'cross' or 'half-cross' belts, for 'shifting' belts, 'cone pulleys,' or for any place where belts are liable to slip, as friction destroys them. * * * * * * * *

"A well-made leather band, if properly looked after—the width and pulley surface proportional to the amount of work to be done—will last 12, 15 or 20 years, and yet be of value to work over into narrow belts."

(To be continued.)

WOODEN RAILWAYS

By D. S. Howard, C. E.

Notwithstanding the retrograde appearance in the use of wooden railways, they are fast forcing themselves into favorable notice. Since the first one built in this country, in 1860, by Mr. J. B. Hurlbert, for the exclusive use of his lumber mill, situated at Hurlbertsville, about five miles north of Boonville, on the Black River Railroad and Canal, public opinion seems to have been very favorably influenced toward them. Mr. Hurlbert has since constructed a similar railway, twenty-four miles long, from the Clifton iron ore beds in St. Lawrence County, New York, to the Ogdensburgh Railroad, which is now in successful operation, and has been examined by a correspondent of the Philadelphia *Press*, which publishes, from his notes, a very favorable account of its usefulness. An agent has also been sent from Canada to examine into its utility, whose report is also very favorable, as republished in the *Portage Lake Mining Gazette*, by which we are informed that a

company has already been formed, and directors chosen, for the purpose of building a similar road from Houghton and Hancock, on Portage Lake, in a northerly direction, twenty-five miles along the Mineral Range, for the use of the mines now open on the route.

There is also a company formed for the purpose of constructing a similar road from Carthage, Jefferson County, New York, northeasterly *via* Harrisville to St. Lawrence County, forty-seven and a half miles, a route well calculated to develop the rich iron ore district about Bonaparte's Lake, near the situation of the Alpine Iron Works, to the Ogdensburgh Railroad. The Black River Railroad, now completed to Lowville, will soon be extended to Carthage. This will be a great inducement for the two companies to iron the extended Harrisville Road, thus completing the nearest route from New York City to the Canada line.

One of the favorable features of the wooden railway is, that not a blow struck in the construction is lost in converting it into an iron road, it being one of the perfect progressive improvements.

Cost of Construction.

The Clifton Road cost $7,000 per mile, through a wild, mountainous region, traversed by deep ravines, rocky ridges, and tortuous streams, one of which the road crosses eleven times in a mile. The grades are necessarily very high, having to rise 1,092 feet in 22 miles. In some places they are 300 feet to the mile. The curves, also, are of very short radius, some of which are not more than 450 feet.

The Carthage and Harrisville Road will probably be built for less than $5,000 per mile. The route is somewhat uneven, but not mountainous, and the two termini nearly on the same level with a surrounding country mostly settled.

The Portage Lake and Keeveenaw Railway route is probably in a more feasible shape still, and, were it not for other considerations, the road could be built much cheaper than the Carthage and Harrisville Road: but as no such work can be done there in the winter season, labor and provisions must be imported every spring for such purposes.

The following figures will show the estimated cost for one mile of the superstructure of the Carthage and Harrisville Road, which will vary in the construction of other roads, as the prices of labor and materials differ in other localities. The grading must be esti-

mated, on all routes, according to the grade required, the character of the material, the contour of the country, &c.

1,760 ties, 3 feet from centre to centre, at 10 c.	$176 00
21,120 feet B. M. 4×6 maple rails, at $15 per M.	316 80
3,520 wedges, at 10 c. each	35 20
192 days' work, 5 rods per day for 3 men	382 00
10 per cent. for superintendence	91 00
Total cost of superstructure	$1,001 00

The grading costs from $2,000 to $6,000 per mile. The whole cost of the first road of the kind built by Mr. Hurlbert, mentioned above, was less than $3,000 per mile, which is less than a good common road could have been made on the same route, with the same grade. The capacity of the two roads, at the same cost of construction, repairs and operation, is as one to twenty-five.

The wooden railway is available at all seasons of the year, and may be made capable of carrying any load required, by increasing the width of the wheel and rail.

Mode of Construction.

Round ties, about one foot in diameter, are bedded into the grade, three or four feet apart, according to the size of rail to be used, or load required. These ties are notched to a proper gauge to receive the rails, which are fastened by driving a hard wood key on the outside of the rail, so that when any part is too much worn or defective in any way, it may be readily replaced. The butt-end of the largest ties are so placed alternately as to receive the ends of the rails where they meet on opposite sides. The meeting of the rails on both sides should never come on the same tie, which, in such a case, has a tendency to rock back and forth in its bed as the load approaches and recedes.

These roads are so eminently useful in so many instances where a limited private business is carried on, such as lumber mills of all kinds, iron works, paper mills, and all manufactories requiring the transportation of large amounts of raw material and heavy products, that we may soon see a marked difference in the success of these various operations, as well as the commencement of many others that could not be carried on at all without such facilities, thereby increasing the wealth of the country in proportion to the dormant energies they thus bring into requisition.

Lyons Falls, December 16, 1868.

NITRO-GLYCERINE.

Public attention has of late been directed to the value, and to the danger in its use, of *Nitro-Glycerine* as an agent to replace gunpowder or gun-cotton in blasting, and several papers have been recently published, that have very elaborately treated the subject.

It is therefore intended to confine the present communication to a simple description of the peculiar method of manufacturing this oil, recently invented by Stephen Chester and Otto Bürstenbinder, and to the statement of some of the facts, that have been developed in a series of experiments made by them, which culminated in their adopting the method of manufacture to be described.

Preliminary to this, a few words may well be quoted from an interesting article by Mr. E. P. North, which was read before the American Society of Civil Engineers, in March, 1868.

"Nitro-Glycerine was discovered in 1846 by Sobrero, but nothing was done with it until 1863, when Alfred Nobel patented its application to blasting. * * * Nitro-Glycerine is made by treating glycerine, which has the formula $C_6\ H_5\ N_3\ O_{18}$ with nitric and sulphuric acids, as in the case of gun-cotton, and the chemical reactions are nearly the same, it being in both a case of the substitution of nitrous acid for a part of the hydrogen. By explosion, according to an article in the *London Mechanics' Magazine*, September, 1865, one volume of oil is converted into

Carbonic acid gas	429	volumes.
Steam	554	"
Oxygen	39	"
Nitrogen	236	"

In all, 1,258 volumes for one volume of liquid oil, being thus theoretically five times more effective than the same bulk of gunpowder."

But it is believed that the effective force of nitro-glycerine in rock blasting is really very much greater than this, as the writer will hereinafter endeavor to show.

The formula for combining the several ingredients from which "blasting" oil is produced, will of course vary with the quality and intensity of acids and oil used.

With such acids and combinations of them as the inventors have

been able to procure hitherto, they have found the best results from the following combination:

By weight—sulphuric acid	6	parts.
" nitric "	3	"
" glycerine oil	1	"

From this the average product will be 1·5 parts (by weight) of nitro-glycerine, having a specific gravity of 1·6.

The amount of oil produced is in great measure dependent upon the relative freedom of the acids and oil used from water, and not upon the chemical purity of the ingredients; and it also greatly depends upon the uniformity of temperature preserved during the process of mixing the oil with the acid. Should the temperature be too high, oxalic acid will be formed, and the heat produced by the sudden commingling of the oil and acid, will at any time cause deflagration. On the other hand, if the oil be but partially congealed (and it freezes at a high temperature), the solution of the oil by the acid will not be complete, and in that case will be dissolved and washed away during the precipitating process.

The inventors have produced from the formula above given, 1·8 parts, while under other circumstances the result derived from the same mixture has been less than ·2 part.

But the affinity of the strongest sulphuric acid for water is so great that the absorption of water from the atmosphere during the mixing, materially affects the product if it be exposed to this contingency.

The peculiarity of this invention exists especially in the method of preserving uniformity of temperature, excluding the atmosphere from the acids, and intimately commingling the oil and acids, and causing the complete solution of the former.

Upon a small revolving table, is placed a jar of capacity to contain, say, twelve or fifteen pounds of the sulphuric and nitric acids previously mixed. In this is suspended a glass tube, with radial branches of diverse lengths, in several horizontal planes, but terminating in fine tubular points, turned to the right or to the left, as the case may be, in such manner that the axes of the capillary orifices in the terminations of the several arms or branches, shall be tangential to the arcs described by the respective arms, from the vertical tube as a centre.

By a tube or pipe, connecting the vertical tube with a reservoir of compressed *carbonic acid gas*, gas is at pleasure forced through

the several orifices described, causing the body of liquid to rotate within the jar, and against the radial arms, while the escaping gas, by reason of its relative gravity, rests upon the surface of the liquid, effectually excluding the moist atmosphere.

But the sudden expansion of the compressed gas within the liquid takes from its caloric, and as the escape is through many small orifices, and not directly upward, the particles of liquid are rapidly and successively brought in contact with the expanding gas, and its cooling effects are immediate and controllable, since a stop-cock at the hand of the operator regulates the flow of gas at will.

On the other hand, great heat is produced by the contact of the oil with the acids. Hence, a similar stop-cock, regulating the flow of the oil into the acids from its reservoir, enables the operator to *immediately* raise or lower the temperature, as the raising or falling of a thermometer suspended in the liquid, may indicate, that one or the other stop-cocks should be turned forward or back.

Practically, it is found that the thermometer may be kept vibrating between two degrees, with but little trouble, while the oil is rapidly intrôduced. Also, that by this means, five pounds of oil may in one vessel be mixed with the corresponding amount of acids in less than forty minutes. It also seems that by this method, that the solution of the oil is immediate, and much more complete than when more slowly mingled in ordinary atmosphere.

This may, however, be due in part to causes other than those alluded to, not yet investigated, but due to the presence and use of carbonic acid gas in the manner described.

The ease with which the temperature is controlled, is such that the operator may readily attend to several vessels, all supplied from the same gasometer, but from separate fountains of oil at the same moment; and as it is found that it is convenient to mix in vessels of sixty pounds capacity, it follows, that under favorable circumstances, one operator can mix what will produce at least twenty pounds of nitro-glycerine every forty-minutes.

This apparatus may all be contained in a box easy of transportation, and which, when the apparatus is taken out, answers for table and ice-box, for cooling the gas cylinder, when the weather is very warm.

After the union of acids and oil is complete, the mixture does not present an oleaginous appearance, and the completeness of the

chemical union can only be guessed at by the clearness of the mixture. This should then be mixed with twenty times its bulk of water, in which it apparently unites, forming nearly a clear fluid, without oily appearance. In twelve hours. however, nitro-glycerine will precipitate, and as its specific gravity is 1·6, will be found on the bottom of the vessels, and the residue of acids and *oil* not previously *consumed* (if this term may here be used) by the acids, may be poured away with the supernatant water.

Hence, large quantities of water are required.

It has been found that after *eight* or *ten* days, decomposition will take place in this product, slowly it is true, but with a rapidity proportionate to the amounts of free acid left. Hence, when it is required to pack and store this material, it is absolutely necessary that after precipitation, as before described, that it shall be many times washed in large bulks of water until no trace of acid remains.

Another curious and important fact has been noted in the experience of the inventors of this method of manufacture. Previous to any decomposition having taken place, it is with extreme difficulty that this oil can be caused to explode, though its expansion *when* exploded seems to be many times greater than that which has been kept for some time. The following experiments will illustrate.

1st. 1½ ounces were placed in a vessel at the bottom of a hole, and over it was placed 2 ounces of rifle powder, in which was inserted a common time fuse, and the whole buried two and a half feet in sand. The powder exploded, forming quite a crater, but the oil was not exploded.

This was repeated several times, with the same result.

2d. Acid and glycerine oil were suddenly dashed into a vessel containing nitro-glycerine. Great heat was developed, and the mixture took fire and burned. There was no explosion Owing to the want of facilities, as these experiments were tried in a sand barren, it was not ascertained whether the nitro-glycerine was consumed, but it is believed that the mixture only burned until the uncombined oil was consumed.

3d. Percussion caps, exploded within the nitro-glycerine, failed to explode it; and it could only be made to explode by confining it, and using extra large cartridges of detonating powder exploded within it.

4th. Equal quantities of this oil, and of some which had been

packed in tin cans (apparently having been thoroughly washed, as no visible decomposition had taken place), were placed in similar positions, but though the old oil could be exploded with great ease, in fact, being very dangerous to handle at all, yet in every case, the effects produced by the new oil seemed to be many times greater than that produced by the old oil.

At Hamburg, in May, 1865, *new* oil, prepared by Mr. Nobel, was poured into an iron cartridge, and the attempt made to explode it by use of common blasting fuse. In all cases, it resulted that so long as the combustible portions of the fuse remained in the oil, that it burned quietly, and ceased burning as soon as the fuse was removed.

A new pistol-barrel eighteen inches long was partially filled with the same oil, and after inserting the percussion fuse, the filling completed with loose sand, and thus prepared, placed in a stout wrought iron gas pipe, three feet long, and two inches in diameter, one end of which was firmly planted in a clay soil. Upon explosion, the earth was lifted several feet high, the gas pipe was torn in three parts, and in the ground a cavity four feet deep, and three feet wide was found, but no piece of the barrel was recovered.

At Wamsbeck, about the same time, a small drill hole in an iron anvil weighing over 300 pounds, was charged with one-tenth of a pound of oil. The anvil was shattered into fragments.

Near Stockholm, in 1865, the following experiments were made with Nobel's Nitro-Glycerine, *newly made*, especially for the purpose of testing the danger of its use as compared with any other explosive compound.

1st. A red hot iron bar was repeatedly drawn through nitro-glycerine poured upon a smooth, flat stone, at first without effect, but finally, when the oil and the surface of the stone became heated, the oil took fire, and part of it burned with a flame, without explosion.

2d. A burning shaving was immersed and moved about in the oil. Though the oil could be made to burn, it ceased to do so when the shaving was withdrawn.

3d. Three bottles filled with the oil, were first heated to 120° F., and were then dashed to atoms upon the rocks, but no explosion took place.

The mass of testimony of American engineers—too voluminous to quote at length—seems corroborative of the fact, that the newly made oil can with difficulty be exploded by violent concussion, accompanied by great heat, and that the extremely sensitive charac-

ter sometimes exhibited, is only developed, after being kept some time, probably influenced by changes of temperature. It also seems that the effective force of the oil decreases in proportion as the sensitiveness to ignition increases.

The object of the inventors, however, has not been the production of an explosive oil of greater strength or better quality than the nitro-glycerine heretofore produced, and though some of the results of experiments tried by them would seem to indicate that it might be so, this is no part of their claim, their object being to render the manufacture of the article so simple and safe that it can at all times be made in all desirable quantities, in the immediate vicinity of the points where it is to be used, thus securing the following direct and unquestionable advantages.

1st. Removing the necessity of packing, storing, and transporting a material which grows more and more dangerous the longer that it is kept.

2d. Supplying a means of manufacture unattended with a shadow of danger, other than in the mere handling of acids.

3d. Dispensing with the labor and time required to thoroughly wash the material, as it may be poured directly from the precipitating jar, into the hole or centre, where it is to be exploded, any time within ten days after the oil has been mixed.

5th. The opportunity afforded to use the oil, when it is unquestionable that its effective force (whether this is due to the process of manufacture, or merely because newly made), is very much greater than when it has been kept or packed, while the danger of handling it is proportionally less, as it then *cannot* be exploded when *unconfined*, and with difficulty (as compared to powder or oil that has been packed), when confined.

HISTORY OF THE INVESTIGATIONS ON THE RESISTANCE OF SOLID BODIES.

By De Volson Wood.

The French have made a very full investigation of this subject, and, in their records, have not been satisfied with merely sketching its outlines, but have entered very fully into its details, and given all the points of interest brought forward by each writer and experimenter. An abridgment of this history, covering 220 pages, is

given in the introduction to *Navier's Résistance des Corps Solides*,* an abstract of which constitutes the following article. The history of the early developments of a science, even in its minutest details, is more interesting than the voluminous productions which trace it in its broader growth. Accordingly, I have given quite a full abstract of the early history, and barely touched upon the later portion.

The architects of the middle ages have not left any traces in their writings of the manner by which they determined the proportions of their structures. All their investigations were upon the equilibrium of external or applied forces, without even considering the resisting forces at the supports, or at the fixed points, and much less the internal resistances of the fibres or elastic resistances.

For instance, they did not consider that *ten-pins* were maintained in a vertical by their proper position, but by the equilibrium of all the external forces.

They were fond of determining the resistance of blocks to crushing; and before using a new kind of stone, they probably made experiments by the aid of levers.

In regard to carpentry, they reasoned upon the combination of timbers which constituted frames; but the low price of timber permitted them to use pieces of large dimensions. But we may presume, however, that they knew that a prismatic piece, loaded transversely, was stronger in proportion to its depth than to its length; for the principal object of Galileo seemed to be, at first, to determine the *theoretical law* of this resistance, as well as the ratio between the *absolute* resistance which is exerted when a piece is broken by a tensile strain, and the *relative* resistance when it is broken by a force perpendicular to its length. After his visit to the arsenal at Venice, he considered the cause of the failure of large machines when small ones of the same kind had succeeded.

At first he considered a horizontal cylinder or paralopipedon, having one end fixed firmly in a wall and a weight acting on the other. He supposed that all the points of the beam in the section of the plane of the wall resisted rupture equally, and hence the horizontal resultant passed through the centre of this section, and was in equilibrium with the weight at the other end of the beam, by means of a right angled lever, of which the point of support was at the lower edge of the section of rupture.

* Resistance of Solid Bodies, with notes and appendix, by M. Barré-de-Saint-Venant. Published by Dunod. Paris, 1864.

The forces being reciprocally proportional to the arms of the lever, he concludes that the *absolute* resistance is to the *relative* as the horizontal arm, or the projecting length of the beam, is to its vertical arm, or half depth of the beam.

From this Galileo easily deduced that a rectangular piece pressed sidewise resisted more than the same piece pressed flatwise, in the ratio of the depth to the breadth of the *section of rupture;* that the weights capable of being supported by pieces which have their ends fixed and the weights supported at the free ends, or whose ends rest on two supports, and the weights supported in the middle, *are as their breadths, as the square of their depths, and inversely as their lengths;* also, *that hollow cylinders, such as bones, quills, the stalks of plants, &c., resist much more to transverse stress than solid cylinders of equal volume,* in a proportion which he determined, which caused him to reflect on the works of creation and considerations of an elevated order.

Finally, flowing from the same principles, he showed that a piece which has one end firmly fixed, and whose inferior surface is a horizontal plane, and whose vertical longitudinal sections are equal parabolas, the vertices of which are at the end where the weight is suspended, is equally strained at every vertical section, from one end to the other. Such a piece is called a *solid of equal resistance,* and gives the most economical distribution of the material.

Galileo's hypotheses were erroneous in supposing that the strains were equal at all the points in the section of rupture, and in placing the support of the lever at the base of the section of rupture; and yet all the theorems which he deduced have been confirmed by later researches, except that which gives the ratio between the resistances to rupture by a longitudinal and by a transverse pressure.

Galileo has been charged with ignorance of the principles which enables us to determine the form of beams of uniform resistance, because he stated that "We may diminish the volume of a rectangular beam more than $\frac{33}{100}$ without diminishing the strength, which is a great consideration in the construction of ships;" but the architect, Francis Blondel, detected the error of this statement, by showing that if the beam is to resist a load applied *at any point,* the longitudinal section should be elliptical; but, in this case, as Navier has remarked, there will be an excess of strength at every section, except the one directly under the load.

The principles of Galileo, as thus understood, were applied by P. Fabri, Vincent, Viviani, and especially by his follower, P. Grandi; but about the same time, Hooke, a noted physicist of England, and Marriotte, a noted physicist of France, announced the basis of the true theory of the rupture of solids.

In 1678, Robert Hooke, in pursuing his studies of steel springs, especially of watches, gave his famous principle *ut tensio sic vis*, which he says he discovered in 1660, but which was first announced in 1676, under the anagram *c e i i n o s s s t t u u.* He based the theory of elasticity on the proportionality of the extensions or contractions to the forces which produced them, and thus explained the vibratory movements of bodies, and added the fruitful remark, *that when elastic bars are bent, the material resists elongation on the convex side, and compression on the concave side.*

Near the same time, in 1680, Marriotte, a Frenchman, having discovered, by several experiments on wooden and glass rods, that the ratio between the resistance to rupture by a pull and by a transverse strain was greater than that given by Galileo, ascertained *that hard bodies, elongated more or less under the action of different weights, and very nearly proportioned to those weights*, and *when the weights were removed they returned to their original length*, and that they are also compressible, so that a stick which is bent contracts on the concave side, and extends on the convex side; and he added, that *it is reasonable to suppose that this compression resists as much as the extension;* finally, the extended parts rupture only because they are *extended* beyond what they are able to resist.

We here see a correct statement of the essential principles of flexure as now understood. Galileo did not recognize any extension of the fibres; and Marriotte supposed that extension always preceded rupture. But the latter made an error in the application of his principles; for he supposed that the resistance to rupture is the same as if the section turned about the base, and the strains increased uniformly from zero, on the lower side, to that producing rupture on the upper side; and hence, in a rectangular section, the resultant will be at $\frac{2}{3}$ the depth, and the total resistance $\frac{1}{2}$ what it would be if all the fibres were equally strained. Accordingly, the *absolute* strength would be to the *relative* as the length to $\frac{1}{3}$ the depth, instead of $\frac{1}{2}$, as found by Galileo. Afterwards he definitely stated *that the axis of equilibrium, on which the fibres are neither extended nor compressed, is at the middle of the depth;* but he still

erred in assuming that the resistance was the same as if all the fibres were all extended. The axis of equilibrium M. Dupin called the *line of invariable fibres*, and Tredgold, and more recent writers, *the neutral axis*.

James Bernoulli, in his writings of 1705, claimed that he was the first to give the true hypothesis for calculating the resistance of solids. Until recently, writers generally gave him the credit of being the first to recognize the compressive resistance. But his theory was the same as Marriotte's, with one unfortunate change, consisting *in regarding the extensions and compressions as augmenting in a less ratio than the forces which produce them.* He endeavored to establish his position by saying, that if compressions were proportional to weights which cause them, a rod would be compressed more than its whole length; and he added that it would be the same with extensions, as they are only negative compressions: and he tried to confirm it by some slight experiments on certain cords, which he found did not follow the law of proportionality rigidly. His reasoning would have been correct, were it not that the elasticity of all materials is changed for a sensible amount of extension or compression.

Bernoulli also erred the same as Marriotte, in affirming that the transverse force of rupturing was the same, whether the fibres are all extended or all compressed, and that it made no difference where the fixed axis is supposed to be, whether at the top of the section or bottom, or at any point between. He failed to perceive the equality of the extensive and compressive forces, and hence could not fix the position of the neutral axis.

In July, 1684, two months after the death of Marriotte, Libnetz, who had learned of his predecessors' investigations and experiments, admitted, like Marriotte and Hooke, the law of the proportionality of the longitudinal stresses, but placed, like his predecessors, the axis of rotation at the base of the section.

Varignon, in 1702, presented a *Memoir on the Resistance of Solids*, in which he placed the neutral axis at the base of the section, and assumed the extensions an unknown function of strains, and deduced a general formula of resistance; then *assumed* different laws, and compared the results with previous experiments, and thus tried to ascertain the most probable one.

The acamedician, Parent, who has not received the notice which he merits, appears to have been the first to make a correct application

of the principles of Marriotte. In his earliest memoirs he made the same errors as his predecessors; but, in a later work, about 1713, he took the moments about the *neutral axis*, and showed that the *sum of the moments of extension and compression gave a result only half the amount found by Marriotte and Bernoulli. He also proved that the sum of the resistances of the compressed fibres would equal that for the elongated ones*, a property before mentioned, and which enables us to establish the position of the *neutral axis.*

Bilfinger, a few years later, gave to Marriotte the credit of being the first to consider compressions; and Coulomb, in 1773, presented his celebrated memoir on the resistance of solids, in which we find laid down nearly all the bases of the theory of the stability of constructions. He established himself on correct principles by using the principle of Statics—then but little known—that the algebraic sum of all the forces must be zero on the three rectangular axes; and the sum of the moments about a fixed point are zero. With the former principles he determined the position of the *neutral* axis; and, with the latter, the ratio between the *relative* and *absolute resistance.* He entirely rejected the perfect rigidity of the material, and considered it elastic, and stated *that if the law of proportionality of extensions and compressions did not hold good up to the point of rupture, the neutral axis would change positions.* No real advance was made in the *theory* for forty years after this essay.

Barlow, in his "Essay on the Strength of Timber," as late as 1817, to determine the position of the *neutral axis,* assumed *the erroneous principle that the sum of the moments of resistances to compression equalled those to tension.* This he corrected in his work in 1837.

Tredgold placed the neutral axis at the centre of *rectangular* sections, but did not seem to understand the simple principles of Parent and Coulomb, which would have enabled him to establish the correct position for all forms of sections.

Navier is one of the most noted authors who has written upon this subject. At first he accepted the statements of Bernoulli and Marriotte, relative to the indifference of the position of the *neutral axis*, but showed that if it be assumed at the base of the section, or at the middle of the depth, that for *flexure the moment of resistance would be as the cube of the depth, and not as the square, as in the case of rupture.* Afterwards, he established an erroneous equation on

the hypothesis that the resistance to flexure was proportional to the curvature of the fibre; and also to its relative elongation, which gave him a compound expression of two terms for the sum of the moments of resistance. But he corrected this expression in his first course to *L'école des Ponts et Chaussées*, and gave a correct expression for the resistance of the fibres when the position of the nextral axis is shown; but, like Duleau, he determined this axis by assuming that the sum of the moments of resistance to compression equaled those for tension. On the 14th of April, 1821, he presented his celebrated paper "On the Laws of Equilibrium and Movement of Solid Elastic Bodies," which was the foundation of *Molecular Mechanics*, or the general theory of elasticity. This was a thorough analytical investigation. In his course, in 1824, published in 1826, he corrected all his previous errors, and was the first to establish the principle *that the neutral axis passed through the centre of gravity of the section when the material is of uniform texture, and the strain is within the elastic limit.* In the same course he brought out more fully the distinction between the law of resistance to flexure and ultimate resistance to rupture, and solved many problems not before attempted. *He also made an essential distinction between gradual and sudden rupture;* but in regard to the latter, it is nearly or quite impossible to assign the law, and is of little practical importance. He also showed how the formulas for a limited strain can be deduced from those for flexure, and added the remark, which has since become very useful, that *it was often better, in practice, to use for safety the elastic limit than a fractional part of the resultant strength.* In the same course he solved the problem of finding the reaction of the supports when a straight piece rests on several horizontal supports and is loaded at different points; *and also the resistance of a piece fixed at both ends, and loaded at any point between the ends.* But one of the greatest steps towards making a practical use of the theoretical investigations of flexure, was made by this noted scholar, in making an approximate solution of the elastic curve. The exact expression of the elastic curve was first given by James Bernoulli, in an enigma, in 1691, and explicitly given in 1694; which is *that the radius of curvature is inversely as the moment of the force producing flexure.* This principle gives rise to a differential equation of the second order, which may generally be integrated once directly and in finite terms; but the second integration is only effected by

quadratures, or elliptic functions, or by series. But Navier assumed that $\text{DS} = dx$ for small deflections, or what is the same, *that the tangent of the angle which the curve of the neutral axis makes with the axis of x is practically unity for small deflections.* This approximation enabled him to make an easy solution of a large class of problems not before attempted. Problems of elastic curves were discussed long before the laws of elasticity were known. Galileo *supposed* that a piece slightly bent was a parabola; and P. Pardis and Laius considered it a catenary. In 1744, Euler made a complete enumeration of elastic curves, and made nine classes, in the first of which the force is nearly or quite parallel to the axis of the piece, and has given rise to much discussion as applied to columns.

M. Persey, in 1834, stated that a single equation of moments about the neutral axis is not sufficient to establish the equilibrium of the exterior and interior forces of tension and compression, unless the neutral axis is one of the principal moments of inertia passing through the centre of gravity. If the resultant of all the forces is not perpendicular to one of the principal axes, the beam will be sprung sidewise. It is then necessary to take two equations of moments of the forces, and it is most simple to take them about the principal axes. If the principle axes of a straight piece—*i.e.*, the axis of the piece is straight, but the sides warped,—are not in a plane, the piece will be twisted at the same time that it is bent.

In 1840, M. Poncelet reduced, theoretically, the danger of longitudinal compression to that of transverse dilations.

Slipping and Tangential Forces.

Beams subjected to flexure experience two other kinds of strain.

1. That which results from slipping of the transverse sections upon each other, and of the slipping, necessarily simultaneous, of the longitudinal fibres upon each other.

2. That which results from tension or the unequal rotation of the sections about the axis of the piece.

M. Vicat called attention to the former in 1833, and called it a *transverse force.* Coulomb, in 1773, gave an expression which could easily be interpreted to mean the same thing. Young spoke of detrusion. Rankine calls it *transverse shearing* and *longitudinal shearing.* The latter is easily calculated when it is evenly distributed over the surface; but it is known to diminish from the centre

to the base of the section. In the practical investigations of flexure, we assume that sections originally perpendicular to the axis of the piece remain normal to the neutral axis during flexure, and this is true only when the flexure is equal from one end to the other, or is the arc of a circle. In all other cases the sections cut the axis at a greater angle than they do the surfaces of the piece, and are warped.

Torsion.

Torsion is to the slipping of a section over its adjacent one, what flexure is to the extension or compression of the fibres. Torsion was studied for the first time by Coulomb, in 1784, and afterwards the theory was discussed by Lagrange, who gave incorrect equations, as was indicated by Binet, in 1814. and which were corrected by Poisson, in 1816 and 1833. Cauchy made an analysis of this subject, and considered the effect of warped transverse sections. His formulas formed the basis of the more thorough analysis of the subject, both theoretical and practical, by Chevandier and Wertheim, the results of which are published in several numbers of the *Annals de Chemie et Physique.* Notwithstanding the exact analytical solution of the problem of torsion is complicated, yet the exact problem of flexure by the mixed method is more complicated, and has been obtained only after long researches.—*Louisville Journal*, 1856.

Resiliance.

To determine whether the limits of cohesion when a body is subjected to a shock is reached, it is necessary to calculate what M. Poncelet has called *résistance vive*, and Young the *resiliance* of the piece or system—that is, to know the live power possessed by the system to resist the blow or shock. To do this we must know the greatest elongation $i=\frac{R_o}{E}$ beyond which it is not safe to pass, and then calculated the total dynamic work of the elastic resistance which is offered by the piece or system. The effect of the shock of a body very *resistant*, such as a spherical shell against a rod, is easily calculated when the weight of the rod is neglected; but in most practical cases the problem is a very difficult one to solve.

Molecular Mechanics.

Without any reference to the practical application of molecular mechanics, the subject has received the attention of the ablest ma-

thematicians since the days of Bernoulli. It would extend this article much beyond suitable limits, to give a sketch of the analysis upon this topic by such noted men as Boscovich, Clairaut, Laplace, Newton, Fresnel, Cauchy, and Poisson.

Their discussions of such subjects as light, heat, capillary attraction, and the like, lead to practical results; but their discussions of the resistance of solids was of little practical value until the principles of elasticity were known and recognized. Of late the conditions of stability are founded upon the limits of elasticity—that is, the strain should not be so severe as to damage the elasticity of the material; but formerly it was established upon the cohesive resistance of the particles. These principles, in some cases, give very different results. In 1855, W. J. Macquorn Rankin gave an article on potential work, strains, stresses, &c., which was published in the *Transactions of the Royal Society* for that year.

The article from which these facts are taken closes with a detailed account of the experiments which have been made upon solids from the time the subject has been treated as a science to the present time; and although reference is made to the *Civil Engineers' Journal* of 1862, yet the author has strangely ignored the theory and experiments of Barlow on the "Resistance to Flexure," published in the *Civil Engineers' Journal* for 1856 and 1858. Barlow, as well as other writers, had observed that the modulus of rupture for transverse stress was not the same as the absolute strength or tenacity of the material when ruptured by tension; and he proposed a new theory, called by him "*Resistance to Flexure*," which was intended to explain the discrepancy. The term I consider unfortunate, for all resistances to bending may be considered a *resistance to flexure*, whereas he intended to include only a certain class of strains. The stress which he considers is the same as that called by Rankine "*Longitudinal Shearing Stress*." Whether his theory be correct or not, the spirit of the article cannot be too highly commended. He presented the results of a large number of experiments, some of which were made by himself, and others were selected, and their agreement with the results as given by his theory, went far towards confirming his views. We can expect to establish the true theory of the resistance to rupture of solids under all the conditions to which they may be subjected, only after a long series of scientific experiments.

University of Michigan, December 3, 1868

Mechanics, Physics, and Chemistry.

THE BEST MODES OF TESTING THE POWER AND ECONOMY OF THE STEAM ENGINE.

BY CHARLES E. EMERY.

Late of the U. S. Navy and U. S. Steam Expansion Experiments.

(Continued from page 55.)

A GOOD dynamometer is the only instrument that can be depended upon to accurately measure the useful work which an engine is capable of performing; still, the best instruments of this kind have many disadvantages for every-day practical use.

In the first place, especially when great power is to be measured, the dynamometer must needs be a large, heavy, and expensive measuring machine, rather than an instrument; consequently, but few can afford to purchase it. The dynamometers, at present in the market, are sold chiefly to establishments that rent rooms with power, where a small machine can be shifted about the building in the night, and so attached as next day to indicate the power used by one of the tenants.

The steam indicator, on the contrary, is neat and compact, and can be easily applied to nearly every kind of steam engine. Its use has, therefore, become so general, that it is acknowledged throughout the world as the standard measure of the power of the steam engine. We have shown the instrument defective, still we cannot point out another, fit in every respect, to take its place. We do say that the dynamometer should always be used to measure the power; but we acknowledge that, in a majority of cases, it is impracticable to apply it. Then, as we have proposed two methods of investigation, one for careful scientific experiment, and the other for practical and tolerably accurate comparison, we conclude that the first would always require the use of the dynamometer, and the latter whenever it is practicable to employ it. Generally, however, until a new instrument is perfected, we must use the indicator alone in ordinary practical trials. It should only be trusted, however, under the circumstances, and subject to the precautions we have before expressed.

When the indicated power alone is used, it is important to know the probable friction of the engine, so that the net power, or that portion available for useful work, may be estimated. A favorite method is to take an indicator friction diagram from the engine, when disconnected from its load, and running at its working speed. The mean friction pressure thus obtained is supposed to be constant at all loads. Hence it is usual to deduct from the indicated working pressure the indicated friction pressure previously obtained, when the remainder represents the force available to produce motion. From this, however, is deducted the friction of the load, usually called seven and a half per cent.; and the net power is calculated from the second remainder. For instance, if the mean working pressure be 42 pounds, and the friction pressure 2 pounds, 40 pounds is available to produce motion without a load; and seven and one half per cent. of this, or 3 pounds, represents the friction of the load; so that 5 pounds pressure is lost in friction, or about twelve per cent. of the whole. This mode of calculation cannot always be depended upon. We have known a case where the mean indicated working pressure in the cylinder was only 8 pounds, and the friction pressure 2 pounds. Consequently, by the above method, about thirty per cent. of the power was absorbed by friction; but the dynamometer showed that less than ten per cent. was lost in that way. Similar cases, differing only in extent, will be found quite frequent. The reason is, that engines are packed for the working, and not for the friction pressure. If the steam pressure be 100 pounds, the packing must embrace the piston and valve rods with sufficient force to prevent leakage, or say 105 pounds for every square inch of surface packed; and nearly the whole of this will produce friction, when a low pressure is used, but the full pressure will work in between the surfaces, and force back the packing, so that the friction from that source will be least when the engine is doing its regular duty. Spring packed pistons modify the friction in the same way. In very large engines the state of the packing would have little influence on the friction, though it certainly would seem proper to loosen the stuffing boxes before taking friction diagrams. In some cases, engines are so weakly constructed, that, though the indicator may show little friction, without a load, there will really be a great loss when the work is being done, due to parts springing out of line, etc. The dynamometer furnishes, therefore, the only true means of obtaining

the net power. In well constructed engines we should be able to calculate the friction by regarding the weight of the moving parts as part of the load, which is moving with a certain velocity in bearings of a given material, and having therefore a certain co-efficient of friction, say seven to eight per cent. For ordinary purposes, when trial is not convenient, we may assume the friction of small engines, of bad design, or of any engine with weak framing, as being from twenty to twenty-five per cent. of the indicated power; while in good engines, of ordinary shape and proportions, it is sufficient to allow fifteen per cent. for medium size, and as low as ten per cent. or even eight per cent, in exceptional cases, in large engines of solid construction and good workmanship.

Having described the instruments used in determining the power of the steam engine, we propose to postpone future remarks upon the proper methods of their application and use, until the closing general discussions; and we will now proceed with the next branch of inquiry; namely:

II. The Economy or Cost of the Power.

Money is the standard unit of value. Hence, everything which costs money, that is required in order to obtain the steam power in any case, is a proper charge to the cost of the power. Therefore, strictly speaking, the cost of the fuel, of the oil, and of needed repairs, together with the wages of the attendants, and also, perhaps, a sinking fund for prospective renewals, should all form part of the aggregate cost. Nor should either of these items be neglected. It would be poor economy for a person to purchase an engine designed to save fuel, which, for any reason was liable to frequent derangement; for it is not alone the cost of the repairs which are to be considered, but the losses which occur from stopping work in the mill or factory. We cannot, however, in our present inquiry, discuss matters of design (though they should always be considered by a purchaser), but must confine ourselves to the methods and means employed to ascertain the economy of fuel.

The combustion of the fuel evolves heat, which uses water as a vehicle, and is carried with it to the engine, and there produces the power. The true measure of the cost, then, is the quantity of heat required to perform a certain quantity of work. Heat being imponderable, can be measured only by its effects on other bodies. The standard unit of heat, or "heat unit," is the heat required to

raise the temperature of one pound of distilled water at 39° one degree Fahrenheit. The mechanical equivalent of a unit of heat is 772 foot-pounds of work; but the best steam engines obtain only about one-tenth of that quantity. Such a result has often been regretted by scientific minds, and many have spoken of it as mysterious. We consider the steam engine of to-day very defective. Some of the defects are inherent; they can be pointed out, but cannot be remedied without changing the general principles of construction. The majority of the practical loss has, however, never been satisfactorily explained. The writer, like others, has his own theories on the subject, but he has no desire to present them publicly till they have been tested; for if they be correct, the principal difficulties can be removed. Few appreciate the extent of the losses in the steam engine. It is only the best examples that utilize even one-tenth of the heat. In such cases, one-tenth is condensed for the work, and about four-tenths is wasted in the clearances and the exhausting steam, even when expansion is carried on, until the terminal equals the back pressure. The remaining five-tenths are imperfectly accounted for. Cases are not unfrequent where only three to five per cent. of the heat taken from the boiler is utilized in work. The discrepancies occur chiefly at the higher grades of expansion. Without expansion, it is easy to understand that most of the heat must go away with the exhaust.

When steam is generated by the application of heat in the boiler, to water only, the water, in becoming steam, always takes up a certain fixed quantity of heat; in other words, becomes saturated with it, and forms saturated steam. Hence, if we can measure the water evaporated, to produce the power of an engine, we can easily estimate the quantity of heat used. The feed water is therefore a perfect measure of the comparative cost of the power, when evaporated in a good boiler, having no superheating surface. The economy of steam machinery is, however, generally measured by the amount of coal or other fuel consumed to perform a certain quantity of work. The conventional standard of comparison between all kinds of engines is, the number of pounds of coal burned per indicated horse-power per hour. The indicated power can be obtained with comparative ease, as has been explained; so also can the coal per hour. Hence the above standard has the merit of great simplicity, and consequently is used by all nations. We must therefore adopt it, or at least use it, in order to be able to compare our results with

those of others; still the method is liable to very considerable errors, which we will examine with the view of correcting them.

It has been shown that the indicator cannot always be relied upon to accurately measure the power. The qualities of coal vary so much, also, in different localities, that the amount consumed does not furnish an accurate comparative measure of the cost of the power. When the coal measure alone is used, too, the engines and boilers are both tested together, which gives no opportunity to ascertain which of the two is entitled to the credit of the performance. This standard will not then answer the purpose of a scientific investigation. In such case, we must ascertain, in addition to the coal, the amount of water evaporated: we can then estimate the value of the coal, and the separate efficiency of both the engine and boiler. The value of the coal, and the efficiency of the boiler, are shown by the number of pounds of water evaporated per pound of coal, and the economy of the engine as compared with that of others by calculating the number of pounds of steam used per horse-power per hour. The weight of the steam used is, of course, the same as that of the water evaporated.

In all ordinary practical trials, the economy must be determined simply by the quantity of fuel consumed to produce the power. Hence, we will first try and find a solution of the difficulties which attend this kind of measurement.

The Fuel.

The different kinds of fuel vary so much in value that it is impossible to accurately compare them. Coal being most generally used, is the natural standard; but there are so many varieties of this necessary article, varying greatly in quality, that it seems a hopeless task to try and compare the performance of steam engines in different parts of the world, or even of our own country, by the consumption of differing coal, which may vary twenty per cent. in heat-producing power. The best way is, evidently, in comparative trials, to use selected coal from the same mine. Yet, how rarely can this be done! and even if this precaution be taken in certain cases, how can a comparison be made with the results obtained by others widely separated, and possessing, possibly, differing views? We must say that the problem cannot be solved with scientific accuracy; still we are able to suggest some corrections which will

reduce all varieties of good coal to substantially the same standard, and thus enable us to use this measure in simple practical trials.

We cannot examine in this paper, with any minuteness, the chemical constituents of the different varieties of coal. For our purpose we will simply divide them into two portions: namely, the non-combustible and combustible.

The non-combustible portion consists, for the most part, of earthy matters, though oxygen and nitrogen gases are often present; and most coals absorb considerable water. The combustible portion consists of carbon and hydrogen, the first largely predominating. In American anthracite about three per cent. of the combustible is hydrogen. The semi-anthracite combustible contains about five per cent.; and the bituminous varieties a larger proportion, varying with the locality of the mines. It is authoritatively stated, that in some varieties of Ohio and West Pennsylvania coal, the hydrogen element is often twenty-four per cent. of the whole combustible. For the consumption of equal weights of hydrogen and carbon, the first requires three times as much oxygen as the latter: the heat resulting should therefore bear a somewhat similar proportion. Favre, Silberman, Andrews, and others, have, from experiment, estimated the calorific value of one pound of carbon to be the heating of about 14,000 pounds of water, one degree Fahrenheit. The corresponding value of hydrogen was similarly determined to be about 60,000 heat units. Bituminous coal, containing considerable hydrogen, should therefore produce very much more heat in combustion than anthracite; but in practice the difference is comparatively small. Mere differences in mechanical structure appear to have a greater influence than chemical constitution. The reason is not evident. The latent heat of the steam resulting from the combustion of hydrogen, which is lost in the atmosphere, will not nearly account for the discrepancy. Without attempting an explanation, except perhaps imperfect combustion, we can, for our purpose, only turn to the records of practical experiments, and find what different kinds of coal have done, and may therefore be expected to do again.

Bourne gives the evaporation efficiency of thirty varieties of coal from different parts of the British Isles, or from 7 to 10·2 pounds of water from a temperature of 212°. The average was 8·7 pounds. These coals are, as is well known, of the soft or bituminous variety. The results of experiments made by the Navy Department, with

thirteen varieties of American anthracite, from different parts of the Pennsylvania coal field, gave a mean evaporative efficiency per pound of coal of 8·9 pounds of water, from a temperature of 212° Fahrenheit. Three specimens of American bituminous coal gave a mean result of 9·9 pounds, under similar conditions. The figures make it appear that our American coals are superior to those of other nations. Professor Johnson, at an earlier period, made some experiments for our Government, with smaller quantities, but obtained more marked results in the same direction. On the contrary, the engineers of the English and French steamers, out of this port, speak of our Cumberland and kindred varieties of coal as inferior to those procured at home. We are in search of the truth, and cannot therefore cater to national vanity. Our best bituminous and clean, free-burning anthracite coals are undoubtedly better than can be found in large quantities in any other part of the globe. All must admit, however, that some of our American bituminous coals are almost identical with the English in appearance and chemical constitution. Both should therefore give the same results, when tested under the same circumstances. In the experiments above mentioned, the English coals comprised a greater number of kinds, the bad being averaged with the good. The United States government experiments were tried with the greatest care, and in a boiler better proportioned for economy, probably, than the average in England. On the whole, we think it fair to assume that the English and American bituminous coals, of the qualities ordinarily supplied to the market, are substantially equal in value, though selected varieties, fresh from our mines, would of course give much better results.

The Government experiments above mentioned showed that the evaporative efficiency of the American anthracite, and the American bituminous coals are in the proportion of 8·9 to 9·9.

The anthracite as a rule, contains much more refuse than the other varieties. The English coals probably average ten per cent. of waste: the West Pennsylvania and Ohio coals have only five per cent., and the maximum of our bituminous coals rarely exceeds thirteen per cent. On the contrary, the refuse from anthracite rarely falls as low as ten per cent., and often reaches to twenty-four per cent., so that, on the average, its waste is double that of the bituminous varieties. It will therefore be interesting for us to examine the results produced by the combustible portions of the different

kinds of coal. The part consumed is called the "combustible," and is found by deducting from the weight of the coal the weight of the ashes, clinkers, soot, etc., which can be collected after the trial. Referring again to the Navy experiments, we find that the mean evaporative efficiency of thirteen varieties of American anthracite combustible was equal to the evaporation of 10·69 pounds of water, from a temperature of 212°, and, for the three varieties of bituminous combustible, the corresponding effect was 10·84 pounds. The results are practically identical. By throwing out of the comparison some of the varieties of anthracite, which justly have a poor reputation in the market, the preponderance would be upon the other side. If, then, we take it for granted that the average foreign and American and bituminous coals are substantially equal in value, the value of the combustible of the foreign coal will equal that of American bituminous and American anthracite, and we may assume that the combustible of the coal, burned in any case, is a tolerably accurate comparative measure of the economy of a steam engine. All these restrictive qualifications are necessary, for if selected coal of the best quality, be used in a trial, the results will be above the average in any case. We wish simply to indicate that the greatest difference in the results given by different coals is due to the difference in the quantity of non-combustible matter, so that, if this be thrown out, the weight of the combustible remaining gives the nearest approach possible, without absolute trial, to the comparative heat-producing powers of different specimens. The best standard to show the comparative economy of the steam engine, other than that of the steam used, is therefore "The number of pounds of combustible used per horse-power per hour."

We cannot, fairly, however, compare the combustible per horse-power per hour, used in experiments here, with other experiments where only the coal was noted. This necessitates us to correct the amount of coal used by a common standard, founded on the combustible. Good bituminous coals, here and in England, have about ten per cent. refuse; hence, to make our experiments compare with those abroad, as well as for convenience, we suggest that in every case, the coal burned in determining the economy of a steam engine be reduced to a common standard of ten per cent. refuse. Let us see the effect of this. The true comparative test for engines is the amount of heat they receive; we have shown that the heat-producing power of the coal is proportioned to the weight of the com-

bustible; hence, if the weight of the coal be also proportioned to that of the combustible, it also expresses the relative economy. The coal is so proportioned when it leaves the same per centage of refuse, so by our plan of correcting the weight of the coal by its combustible, so as to give ten per cent. refuse in each case, the weight of the coal is a true comparative test of the relative economy of the engine. For instance, 100 pounds of coal leaving twenty per cent. refuse will evaporate no more water than 88·9 pounds leaving ten per cent. refuse, for both contain only 80 pounds of combustible. If to the combustible we add one-ninth of its weight, the quantity added is one-tenth, or ten per cent. of the sum, which represents the weight of the coal, corrected to the uniform standard of ten per cent. refuse. Suppose a horse-power in a certain foreign steamship costs 2·8 pounds of bituminous coal per hour, and in an American vessel it costs three pounds of coal, using anthracite, are we to say our engines are inferior? Let us see. We first deduct the refuse from the anthracite—for instance, twenty per cent., which leaves 2·4 pounds of combustible. This, then, is nine-tenths of the weight of coal having ten per cent. of refuse: so multiply 2·4 by $\frac{10}{9}$, gives 2·67 pounds as the true cost of the power in the American engine, to compare with 2·8 pounds used by the foreigner, when both are compared by the same standard.

We have been thus explicit, because the fuel is so generally used in the comparison of the performance of steam engines. The coal bills of course show the absolute cost of the power in any particular case, no matter what quality of coal was used; but, under such circumstances, the weight of coal consumed, even when corrected as above pointed out, is, as must be seen, but an imperfect *comparative* measure. To make comparisons sufficiently correct to answer the demands of science, we must measure the steam used in each case—in other words, compare engines by the number of pounds of steam used per horse-power per hour.

The calculations are usually made from the pressure shown at the termination of the stroke; the assumption being that the engine uses, at every stroke, one full cylinder of steam at that pressure. In other cases, however, the initial pressure, and the portion of the cylinder filled at the point of cut-off, are used in the calculation. These methods of determination pre-suppose that dry or saturated steam enters the cylinder, which may be true, and that the steam continues in this state, through at least part of the stroke, without

condensation, which is never the case. Steam is necessarily condensed to set free the heat transmuted into the work done; and the temperature of the metal of the cylinder is a mean of the temperatures to which it is subjected, and therefore the surfaces form a condenser with respect to the initial steam. The consequence is, that there is always more steam taken from the boiler than is shown by the indicator: the discrepancy increasing with the degree of expansion and amount of external refrigeration. Clarke, in his work on the locomotive, points out great differences between the amount of steam calculated from the initial and terminal pressures shown by the indicator; and yet uses the first in all his calculations. Later experiments, where the steam has been actually measured, show that in small engines twenty to thirty per cent. of the steam is unaccounted for by the indicator at full stroke: and as high as sixty to eighty per cent. when the steam is expanded considerably. Large engines show a small discrepancy at full stroke, which rises to thirty, and often fifty per cent., with shorter admissions. The best examples of the English double cylinder pumping engines with steam-jacketed cylinders use thirty-three per cent. more steam than is shown by the indicator on the cylinders. This method of determination is therefore absolutely worthless for our purpose, as it furnishes no basis for reliable comparative tests. These discrepancies show us where a great loss takes place in the use of the steam engine. They have been ascertained, in practice, by indicating the engine and measuring the water pumped into the boiler, and evaporated there, to furnish steam. In other cases, the exhaust steam of the engine has, by surface condensation, been reduced to water, and its quantity determined by measuring or weighing it. The weight of feed-water, or what is the same thing, of steam used in any case, to produce a given power, may, by either of these plans be ascertained with scrupulous accuracy; and if the coal be weighed at the same time, the evaporative efficiency of the boiler can also be determined, and the excellence of both engine and boiler be detected and credited aright.

In addition to the standards above given, expressing the economy of the engine, others of special application are used, which give the cost in terms of that for which money is paid, namely, the coal, and the result in that which returns the money. For instance, the miller speaks of the number of pounds of coal it requires to grind a barrel of flour—a thing, by the way, which may depend as much

upon the condition of the mill as of the steam machinery. Locomotives are rated by the number of pounds of coal or coke burned per ton per mile. So, also, what is known as the "duty" of a pumping engine, is the number of foot-pounds of work derived from the consumption of a certain quantity of coal.

(To be continued.)

SPECTACLE GLASSES FOR PUBLIC SPEAKERS.

By S. W. Robinson.

It has been set forth by John B. Gough, the great lecturer, that a public speaker should wear nothing that will throw flashes. This truth appears self-evident on enunciation, and should be particularly regarded by all public speakers. A glistening button, watch-chain pendant, or breast-pin is always noticed. The richest ornaments, particularly those set with diamond jewels, are worst of all. But what can be more effective in projecting flashes than spectacle glasses as now constructed, especially those of slight convexity, such as should be adopted by persons beginning to wear them? First, perhaps, we see the natural eyes of the speaker; then suddenly, two fire-balls take their place, of demon-like appearance, winking alternately, or in concert, to the turn of the head. The object in view in the few words now offered, is to propose a remedy, or a partial one at least, for this inconveniency regarding spectacle glasses.

Light reflected from a plane surface, as large as a spectacle glass, proceeding from the blaze of a gas burner, will appear to an observer, as far from the glass as the glass is from the burner, to proceed from the whole surface of the glass when the position of the latter is favorable; because, under these circumstances, it is only necessary that the blaze of the burner should have, as it very nearly does, twice the breadth of the glass. If the surfaces of the glass be slightly curved, it is only necessary that the burner be comparatively a little nearer to it. The effect is still worse when the light reflected, proceeds from the broad surface of a window; and the spectacle lens may then appear fully illuminated with considerable convexity. This not only produces an annoying flash, but ever and anon perfectly obscures the eyes of the speaker wearing the spectacles. But the greater the convexity of the lenses of the spectacles, the smaller will the reflected image of any given object appear. If this be carried to a sufficient extent, the images

of a whole window even may be reduced to such an extent as to cause no serious inconvenience, any more than the images of the same objects as ordinarily reflected from the cornea of the eye itself. To secure the desired degree of convexity of the two surfaces of the lenses of the spectacles, without adding to the magnifying power, it will be necessary to grind the lenses in the form of the meniscus. This will require some additional trouble on the part of the practical optician; and such spectacles would consequently be more costly. But cannot, and would not many of our public speakers, such as clergymen and lecturers, avail themselves of the use of such glasses if they could be had, even at a considerably extra expense. Let opticians give their attention to the production of a few such spectacles, if for no other purpose than to try the experiment, believing that if they can be produced without a greater sacrifice than the object gained, that they will thus become contributors to the relief of a present serious want.

This form of spectacle will also possess some incidental advantages. When the eyes are turned considerably to one side or the other, the rays of light will reach them by a more direct passage through the substance of the glass, and thus the eye receive more light if the glass is not perfectly pure and clear. The frame of such glasses can also be worn nearer to the eyes, and give the same amount of room in front of them, so as to admit of a broader range of lateral vision without interference with the frames of the lenses, or passing outside of them altogether.

It would seem that the eyes, thus favored, with a clear and unobstructed view, could almost believe themselves enjoying youthful days.

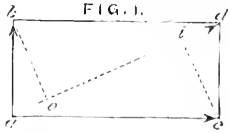
University of Michigan.

COMPOSITION AND RESOLUTION OF FORCES.

By John C. Trautwine, C. E.

The following illustration of the composition and resolution of forces, is familiar to everybody; but it appears to me that the deductions that are drawn from it by even eminent authorites, are *entirely erroneous*. Let a b c d, Fig. 1, represent a floating cake of ice. Upon it a man starts from a to walk to b, on the opposite shore; but during the same time,

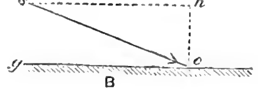
FIG. 1.



the ice floats from *a* to *c*; so that although actually walking along only the line *a b*, he arrives at *d*, instead of at *b*; having really traversed the line *a d*. Now the line *a b* may be taken to represent the force which he expends upon himself during his walk; and *a c* that expended upon him by the ice in the same time. The savans tell us that since he has actually crossed the stream, which is supposed to be as wide at *c d* as it is at *a b*, no part of the force *a b* can have been lost; and again, since the ice has carried him as far down the stream as it has itself moved down, therefore no part of the force *a c* has been lost. Hence, they say the resultant *a d* actually represents the united wholes of *a b* and *a c*; and this is accounted for on their beautiful (ridiculous?) principle of "the independence of the simultaneous action of many forces upon the same point;" from which says Morin* "it follows *quite naturally* (the italics are my own), that the forces which produce these motions, exert actions independent of each other. Some compare this independence to the crossings of different circles of little waves; as if these did not affect each other. My own impression is, that so far from being *independent* of each other, they actually (not only in this case, but in all others), in a great measure *destroy* each other; and that it is only their *remains* that constitute the resultant force, *a d*. The force *a b* may be conceived (as is perfectly well known to the veriest tyro in mechanics), to be made up of two forces respectively, equal to *ao* and *ob*; and the force *a c* in like manner, of two equal to *a i* and *i c*. When the forces *a b* and *a c*, come into collision in the man, the two subcomponents *o b* and *i c*, cannot take any part in *moving* him; for being equal, and in diametrically opposite directions, they react against, counterbalance, or render null, each other's action; they become *static* force. They *can do* nothing more; and they *must do* that.

If to the body B, Fig. 2, we apply an oblique force, *s o*, (of which the components are *g o* equal to *s h*; and *h o*,) we know that the component *h o*, at right angles to the surface *g o*, encounters the equal opposing force of B; against this it reacts; and is thereby rendered static, or incapable of producing motion. But the component *g o*, equal to *s h*, still remains as moving force.

FIG. 2.

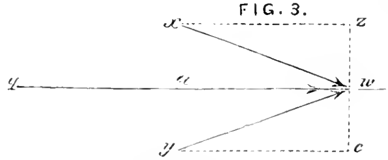


In the same manner, if we apply two forces, *x w* and *y w*, Fig.

* See Bennett's Translation, page 132,

3, to a man, or to any other body, w, whether on floating ice or on land (unless there is some mysterious counteracting principle in floating ice, which I am unable to comprehend), the two sub-components, $z\,w$, $c\,w$, will become static; and only the two which are equal to $x\,z$ and $y\,c$; *or to the resultant*, $q\,w$; will remain as dynamic or moving force, along the diagonal, of the parallelogram of forces, to move the man. Depend upon it, the principle of the "independence of the simultaneous action of two or more forces upon the same point," (even as applied to the heavenly bodies) is a fallacy; although Morin, page 119, cites a most delicate and conclusive experiment by M. Tresca, sub-director of the Conservatoire, in support of it. A resultant is not a measure of the forces represented by the components; it is only a measure of its own force; which is always less than those represented by its components. In statics it represents a force, which if reversed in direction, so as to become an anti-resultant, will balance the *remains* of the components; and in dynamics, it represents the smaller moving force left in the components after they have partly rendered each other static. No *practical* error, however, will arise from saying as usual in all cases, that the resultant is a force which, if reversed in direction, will *balance its components*. That an error *in science*, however, has arisen, is I think clearly shown by the necessity for inventing the "independence" theory, to explain away the supposed mystery, alias adopted fallacy.

FIG. 3.

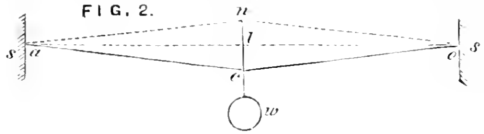


Since the resultant, so-called, of two oblique moving forces, is in fact the result of their *remains* only, it is plain that the components can in no way, whatever, be regarded as representing either the resultant, or its effects. For although two components *diminish each other* in producing the resultant; the resultant *cannot increase itself* so as to produce the components. Two oblique moving forces, which we will call 10 and 20, may so diminish each other by reaction, or straining against each other, in their efforts to change each other directions, that their remains, resultant, or diagonal force, will impart to a body a motion of but 5. But if that motion (or rather that moving force of 5 which produces that motion), be reversed, or turned back on its course, it plainly cannot impart a motion of 10 to one-half of the body; and a motion of 20 to the other half.

Whatever the astronomers and other savans may say, *force* IS *always lost in changing the* DIRECTION *of force;* and the two components represent two moving forces to which the resultant would be equal, *if it was large enough*.*

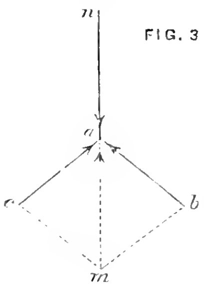
But it is intimated at times that a resultant actually *does increase itself;* thus we are told that if at the centre, *c*, of the rope, *a c o*, Fig. 2, attached to two walls, *s s*, we apply a weight, or force of gravity, *w*, of one ton; represented by the resultant, or diagonal, *n c;* the said force of one ton will produce two forces, *a c*, *o c*, each (per figure) of four and a half tons; or together, nine times as great as the resultant force, *n c*, itself, which produces them. But this is altogether a mistake; for the fact is, that the one ton has nothing to do with producing these nine tons; they are produced entirely by the walls. The force, *a c*, is the resultant of *a l*, and one-half of *n c;* and *o c* in like manner, of *o l* and the other half of *n c;* and all these are derived from the walls *exclusively.* The one ton contributes an entirely distinct vertical force only equal to one ton.

FIG. 2.



I will endeavor to make this clearer; and to show that force, whether as *motion* or *strain*, cannot produce components greater than itself. I will premise that I call a force, *n a*, Fig. 3, which is equal to a resultant, *m a;* and diametrically opposed to it in direction, an *anti-resultant.* Now let *c a* and *b a* be two *moving* forces; *m a* their resultant; and *n a* their anti-resultant. Taking it for granted that the reader already sees that although *c a* and *b a* may partially destroy each other, so that the resulting motion would only be one from *a* to *n;* and also that a motion from *n* to *a*, could not reproduce the destroyed parts, so as to restore two forces, *a c* and *a b;* I will proceed to *straining* (pulling or pushing, or reacting) forces. Let *c a*, *b a* push against *a*. Their resultant, or their remains, is a push equal to *m a*. Now, will an anti-resultant push, *n a*, produce the two pushes, *c a*,

FIG. 3



* A resultant has been defined to be a single force, which if reversed in direction, would balance *its components;* it should be *the remains* of its components; for a resultant if reversed can balance no forces amounting to more than itself.

b a? The reader will probably say yes. I say no. *We, ourselves, must absolutely provide forces in their proper positions, before n a will push at all;* and we must provide at least as much as *c a*, *b a* lost in producing *m a*. I repeat, *n a* cannot push at *a* at all, unless *we* first put forces there for it to push against; it cannot make forces for itself to push against. When we apply two oblique forces to produce *motion*, we must take care that there is no third force present to prevent said motion; but when we apply them to produce *strain*, we must also apply a third force for them to strain *against*.

All this will probably be better understood after reading what is said in reference to Fig. 5.

There is a prevailing idea that one force, however great, tending in one direction, can oppose no resistance to another force, however small, tending to move it at *right angles* to that direction. This is a palpable error. The doctrine is supposed to be confirmed by such illustrations as the following:

If a rigid (unchangeable) body weighing but an ounce, be squeezed horizontally between the jaws of a vise, with any amount of force whatever, say, 1,000 tons, then this immense *horizontal* force will not tend in the slightest degree to prevent the *vertical* one ounce of gravity from moving the one ounce body downwards, from between the jaws of the vise. It is true that the body will not so move; but its motion is prevented by the reaction upwards of one ounce of a third force, *friction;* which is generated by the pressure of the vise.

Now, this is all very correct; but unfortunately it has no connection whatever with the subject; for the vise presents no force tending to move the body horizontally, or at right angles to its vertical direction. On the contrary, it presents *two equal and opposite forces;* either one of which *would* move the body horizontally were it not that the other tends *equally* to move it horizontally *in the opposite direction*. These two tendencies to motion, react against, strain, or destroy each other against the internal cohesive *forces* of the particles which compose the body; but upon the *matter of the rigid body itself*, that is, upon *the particles themselves, they produce no effect whatever.*

If the body is not rigid, and if the cohesive forces of the particles are not sufficiently great to withstand the reacting forces of the vise, then the particles will be displaced; the body will be reduced

to fragments, and the overplus of the reacting forces of the vise having no longer any opposing forces to *strain* against, will be converted into *moving* forces, and will set the fragments into motion, as so many separate bodies, in all directions.

I suggest the following as a safe principle. If it has been before laid down, I have not seen it, namely: that reacting forces cannot affect a moving one; and, vice versa, that a moving one cannot affect reacting ones. The non-recognition of this broad principle, has, in my opinion, been the cause of much error, not only among practical men, but among some authorities on such subjects. I will merely offer one homely illustration, the simplicity of which is better calculated to impress the fact upon the memory, than one drawn from a more dignified source would be. Suppose the vise and the squeezed ball between its jaws to be let fall together. It is plain that the squeezing or reacting forces of the vise will not cause the moving force of the ball to become either faster or slower; and equally plain that the downward moving force of the ball will not cause the reacting forces of the vise to squeeze it either more or less. I would direct attention to a point which appears to have escaped the notice of writers. When one, as $g\,d$, Fig. 4, of two oblique moving forces, $g\,d$, $h\,d$, applies at one point, d, forms an angle, $g\,d\,m$, with the diagonal, $m\,d$, greater than 90°, or a right angle, then lines $g\,y$, $h\,t$, cannot be drawn from the angles g and h, at right angles to the diagonal itself; but the diagonal must be extended equally both ways, as to y and t, for that purpose; and when this is the case, the lines $g\,y$ and $h\,t$ no longer measure those portions of the two forces which react against each other, and against the internal cohesive forces of the body; but $g\,y$ and $y\,d$ on one side, and $h\,t$ and $t\,m$ on the other, thus react; leaving, as before, only the diagonal or resultant, $m\,d$, as the measure of the *remains* applicable as motion, to moving the body, d, itself, as a whole. Hence, in such cases, the loss which each force undergoes, is no longer in proportion to the sine of the angle which said force forms with the resultant. Thus if $g\,d\,h\,m$ be the shape of the cake of ice, instead of Fig. 1, and if the man walk the line $d\,g$, while the ice moves from d to h; then the man's own force from d to g (composed of $d\,y$ and $y\,g$), would be reacted against

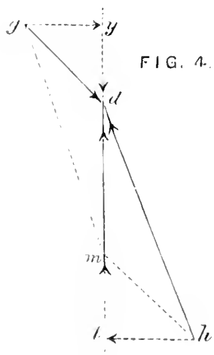
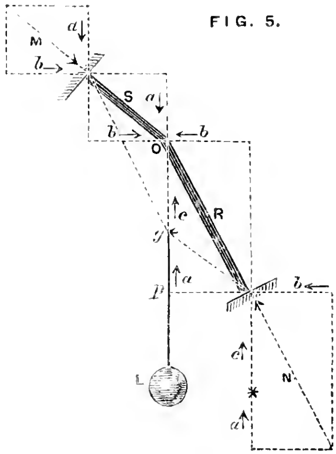


FIG. 4.

and entirely destroyed (so far as regards the man as a whole), by *m t* and *t h*, of the force *d h* of the ice; and the man would be found at *m;* having been carried there by *d m*, the remains of *d h;* and in fact of all the motion that remains from both the forces; the other portions having acted as *strain* against *each other in* the man.

In Fig. 5, let S and R be two rafters, sustaining a load, L, represented by the resultant, or diagonal, O *g*. Now, here there is no motion; what effects does this load or vertical force produce upon the rafters? We are told that L is supported by the rafters; and that it produces in them the strains, S and R. But this is incorrect in two particulars; for it is in fact supported by the walls; and, moreover, one force cannot produce strains. We will omit the consideration of the weights of the rafters themselves; or will assume them to be without weight; and then the case becomes one of three straining forces represented by R S, and O *g*, all applied at once at the point, O. The force along S is furnished by the wall, M; that along R, by the wall, N; and that in the direction from O to *g*, by the load, L. The rafters are merely mediums for conveying the forces M and N from the wall to O, as the rope conveys that of L to O. These forces of course respectively pass through the entire lengths of these mediums, each of which is consequently strained from end to end.



The force, M, may be supposed to be the resultant of the vertical and horizontal components, *a* ↓ *g* and *b* →; and the force, N, of *a* ↑, *c* ↑, and *b* ←; and these, together with the vertical force, O *g*,* may all be considered to be applied to the point, O, as in the figure.

This being done, we see that *a* ↓ and *b* → of the force of the wall, M, are reacted against or balanced by *a* ↑ and *b* ← of the force, of the wall, N; while the vertical, O *g*, is reacted against or balanced

* The force O*g* being already vertical, cannot be resolved into two components, one of which shall be vertical (or in its own direction); and the other in another direction. In other words, the only component a force can have *in its own direction*, is itself.

by *c* † of the force, N. Therefore, the three forces, S, R, and O*g*, precisely balance each other. I ask particular attention to the strain measured by the vertical line, *p o*, when one of the forces, as S, forms with the diagonal, O *g*, an angle, S O *g*, *greater than* 90°. Since this is one of the components of the strain along R, it shows that the rafter, R, actually transfers to the wall, N, *a vertical pressure greater than the weight of the entire load*, L, which is represented by the part *g* O only, of *p* O. At first sight, the possibility of this will probably be denied by the practical reader, who, deceived by the phraseology frequently employed in reference to the parallelogram of forces, will exclaim that a load cannot possibly produce a vertical pressure greater than itself. That is true; and the fact that here IS a vertical pressure greater than the load itself, is conclusive proof that it is furnished by the *walls*. The excess, *p g* of the strain, *p* O, over or beyond *g* O, is caused by the two reacting forces, *a* ↓ and *a* †, of the walls, M and N. I have never seen this fact clearly laid down in any book.

ON THE NATURE AND DISTRIBUTION OF GOLD IN METALLIC SULPHIDES.

(A paper read before the Polytechnic Association in New York. December 24. 1868.)

BY DR. ADOLPH OTT.

IT is a common belief that wherever gold is found in pyrites, the same is not contained in it in a metallic state, but combined with sulphur, or, according to others, with arsenic or antimony. This view is most generally supported by the fact that, in desulphurizing auriferous pyrites or exposing them to the action of the atmosphere, a larger quantity of the precious metal is generally obtained than if the ore is directly washed or amalgamated; and it furthermore is supported by the fact that the gold, in most cases, only becomes visible after desulphurization.

Though none but these facts are before us, which would substantiate this view, the theory of the gold being present as a sulphide, gained recently, nevertheless, a large number of adherents by the discovery of a mineral consisting chiefly of the sulphides of osmium and ruthenium. The same is found in Borneo, and has the appearance of crystalline iron. It was urged that if these

metals, which so much resemble gold in their chemical actions, occur in combinations with sulphur, it may be equally the case with gold. It was, however, not taken into account that though the existence of a natural sulphide of gold is not beyond the limits of possibility, it does not follow from the occurrence of that newly discovered sulphide, that a sulphide of gold is also present in the pyrites.

Adherents to the sulphide-theory are not only most of the miners and metallurgists of America, but also authorities like Dumas, Brogniart, Erdmann, and, in this country, Emmons, Lieber, Renwick, and Stevens, savants who are more or less known by their geological researches. Dumas, in behalf of his views, quotes the above mentioned observations, and states the fact that, by the aid of the microscope, he was unable to discern metallic gold in finely crushed auriferous ores; and also expresses the opinion that the gold, because of its electro-negative nature, must mostly be present as a compound sulphide.* So does Brogniart. Erdmann gives to the auriferous arsenical ore of Reichenstein, in Silesia, the formula: $2\ (An^2\ S^3),\ 3\ (As^2\ Sb^2\ S^3)$;† but upon what ground he bases this formula is unknown to us.

Without entering here into the profound researches of Professor Henry Würtz on on the "Genesis of Gold," where this savant defends the opinion held by us, that this metal can only be mechanically diffused in its ores, we shall consider some facts which are before very one.

As concerns the circumstance mentioned by the adherents of the sulphide-theory that more gold is obtained by first roasting the ore, instead of amalgamating it directly, it may be remarked, that this cannot be considered as a proof of the non-metallic nature of the gold in the sulphides; for, if we consider that this metal is generally contained therein in a finely-divided state, and that by crushing the ore to an ordinary fineness, a large portion thereof will always be imbedded in particles of pyrites, the above fact is easily explained. We obtain, moreover, by a repeated process of crushing and amalgamation, always a new portion of the precious metal; and in some not very rare cases, a yield is obtained by this treatment, which comes very near to that of the assay. The same results have been obtained by the washing process. We also would

* Muspratt's Chemistry, Vol. II., p. 264.

† Bruno Kerl, die Rammelsberger Huttenprocesse, p. 4.

refer to the fact that, by the amalgamation of the quartz, as well as of the pyriteous ores, rarely more than 70 per cent. of the precious metal is obtained. Prof. B. Silliman, in one instance, was unable to extract from California quartz, without the use of sodium amalgam, more than 60 per cent., and, in another not 40 per cent.*

Gold which is not visible by the unassisted eye, often not only occurs in sulphides, but also in quartz. In carefully treating it, such ore generally pays very well, while veins with auriferous ores, which furnish fine specimens for mineralogical cabinets or stock companies, are, on the average, not very profitable.

Again there is another circumstance which may sometimes contribute to the fact that a preliminary roasting of the pyrites proves more satisfactory than direct washing or amalgamation. In roasting finely-crushed auriferous pyrites from Virginia, Prof. Silliman has found that the gold which was hardly visible by the unassisted eye, was generally deposited at the top of the ore. In consequence of this, it was easily washed away, while the black sand which accompanied the quartz generally settled at the bottom. Silliman ascribes this property to the nature of the gold; while the latter existed in flattened particles, the black sand, titanium(?) occurred in angular pieces.† In such cases, *i. e.*, where the gold is met with in foliated particles, it is not quite improbable that in roasting the ore they melt, and then, overcoming the adhesion by which they floated on the top, sink to the bottom. We know well that the temperature in the desulphurization of auriferous ores is high enough to effect a volatilization of the precious metal. In any case, it is certain, that by the high temperature disintegration takes place, and thus an increase of the difference between the specific weight of the ore and that of the gold is effected.

As regards the second argument advanced by the adherents of the sulphide theory, *i. e.*, that the gold only becomes visible after a preliminary roasting, it finds its explanation in the disintegration which the pyriteous particles that surround the gold suffer by the heat. The hypothesis of a gold compound appears, therefore, quite unnecessary. Metallic gold is, besides this, sometimes visible in undecomposed pyrites with and without the assistance of a magnifying glass. In disintegrated pyrites, it often appears in

* *Chemical News*, 1866, Vol. XIV., p. 170.

† *American Journal of Science and Arts*, Vol. XXXII. p. 98.

the forms of scales and grains, which could not be the case if it was contained therein in a chemically combined state. This has also been observed when the the sulphides are dissolved in nitric acid, which fact was already known in the beginning of this century. Joseph Black, in his "Lectures on Chemistry," Vol. III., p. 384, mentions the following:—

"When the Hungarian pyrites is dissolved with aqua fortis, it is said that the gold is left, by that acid, in the form of minute atoms and fine films, which are in a metallic state. And as this pyrites varies much in the quantity of gold it contains—and some of it does not contain any—there is reason to believe that all the gold in it is metallic and pure, and only dispersed through it in very minute films not mineralized."

We are so fortunate as to meet with the following passage in Fourcroy's "General System of Chemical Knowledge," Vol. VI., p. 489, which was published in 1804.

Bergmann (1735—84) observes "according to the examination of several auriferous pyrites, that the gold which is extracted from them by digestion in nitric acid, is in small angular grains, which prove that this metal existed in the state of simple mixture, and not of composition in the pyrites. Thus the ores of copper, silver, lead, iron, cobalt and antimony, from which gold is frequently extracted by docimastic and metallurgical operations, appear to contain this metal simply disseminated amongst their particles; and there is reason to believe that when these ores are decomposed, effloresce, become sulphatized and dissolved, the gold which separates from them is then carried away by the waters and deposited in the sand as if it were native gold."

In treating auriferous sulphides of Colorado, which, to the magnifying glass, did not reveal a particle of gold, Prof. Torrey of the U.S. Assay Office, has also shown that, by treating the ore with pure nitric acid, the gold appears under the microscope in laminæ, and in filiform and spongy particles.

If, finally, we consider that the gold occurs in nature in combination with rare metals, as with tellurium and silver in the sylvanite, and alloyed with mercury and bismuth, but not combined with sulphur, we necessarily must refer the hypothesis of the "gold sulphide in the pyrites" in the realms of phantasy.

It is well known that we seldom meet with a single sulphide in nature: they generally appear associated together; galena, for

instance, often occurs with zinc blende, copper and arsenical pyrites, with iron pyrites, &c.

The distribution of ores in veins being a very different one, it seems that this is also the case with that of the gold imbedded in the various sulphides. The questions therefore arise:—

1. Whether the gold is distributed in those ores uniformly or not?

2. Which of the sulphides seem particularly to be the matrix of that metal, and in this case?

3. Which of the respective sulphides is the matrix?

We possess almost nothing but suppositions on this subject. According to Hausmann the gold in the Rammelsberg is probably associated with zinc blende, as this is the case in the Lautenthaler veins;* he concludes this from the fact that in the galena, as well as in the iron and copper pyrites from the Oberharzer veins, which are free from blende, no trace of gold can be discovered. Kerl, however, is of the opinion that because the zinc blendes of the Juliushutte yield the smallest, and the copper ores which are rich in iron pyrites, produce the largest amount of gold, the iron, and perhaps in a still higher degree, the arsenical pyrites, are to be considered as the matrix for the gold, aside of the zinc blende.† In the ores of the Rammelsberg, the copper pyrites are, according to Holzmann, the matrix of this metal.‡ In this country, the copper pyrites are also generally considered as the matrix of a large quantity of the gold occurring in the sulphides. This condition has especially been met in Colorado, where, according to Withney, some beds of this ore often yield over $2,000 per ton. There the Chilean proverb may therefore be regarded as true: "If thou findeth copper, thou hast gold." In North Carolina, Virginia, and elsewhere, the auriferous veins lead, in depth, to copper veins.

* Hausmann, Studien des Gœttinger Vereins bergmænnischer Freunde, Vol. III., p. 332.

† Bruno Kerl, Ergænzungen zur ersten Auflage der Rammelsberger Huttenprocesse, p. 22.

‡ Holzmann's hercyn. Archive, p. 525.

EDUCATIONAL

SUNLIGHT AND MOONLIGHT.

A Lecture delivered at the Academy of Music, before the Franklin Institute, on May 23d and June 6th, 1868.

By Prof. Henry Morton, Ph. D.

(Continued from p. 69.)

Having considered the evidence which astronomical observation affords as to the source of solar light and heat, we will next pass in review, the proofs we possess concerning those details of structure which we have before described.

The first of these is that supplied by *sun-spots*. Seeing spots in the sun has obtained a proverbial significance, for excess in unamiable criticism, and we might thus suppose that the sun itself is in fact spotless; this is, however, so far from being true, that it is not often possible to find the sun free from spots, or seemingly black markings, when it is viewed through a telescope; while on some occasions, these spots have been sufficiently large to be observed by the unaided eye. To describe these appearances more minutely, we should say that they were irregular dark markings, with a black central portion appearing at one edge of the sun, traveling across his disk in parallel lines having a small inclination to the ecliptic, in the space of about fourteen and a half days, disappearing on the further edge, sometimes finally, at others only to reappear fourteen and a half days after, on the same side as at first, and repeat their journey in the same time as before; in some instances as often as eleven times, consecutively.

As they pass across the solar disk, these spots undergo various changes; in the first place they increase in diameter, or broaden, in the direction of their motion; they also alter their shape in an arbitrary and irregular manner, and besides, occasionally exhibit a rotary movement. Their motion across the disk and widening in that direction, naturally suggests and is explained, by the supposition that they partake the general rotary motion of the sun

(which we thus conclude to be complete in twenty-five days, due allowance for the motion of the earth being made), and owe *this* change in their form to his spherical shape.

As these spots appear on the one side, it is the border of lighter shade which is first seen, and the central spot comes afterwards into view, while as they again disappear on the other edge, it is the border or penumbra, which seems to close over the centre, or umbra, just before the spot disappears. This shows us that these spots cannot be projections, but must be depressions on the solar surface.

It has been observed by De La Rue, Stewart and Lowey, that in a majority of cases, the "following" edge of the spot is more luminous than the surrounding surface; thus seeming to indicate that the luminous matter of the photosphere is especially accumulated on this part, which would be the result had that luminous matter been driven upwards through the space occupied by the spot, from a more central and therefore less rapidly moving region. The spiral shape and individual rotary movement of many spots, suggests to us at once an analogy with our tornadoes, cyclones, or rotary wind storms, and we are thus naturally led to consider the sun-spots as great rents in a canopy of luminous cloud torn asunder, and hurled up and outwards by a rush of heated gas, such as it would be most natural to suppose the heat locally developed by the impact of an infalling meteoric mass, would produce.

The extent and rapid change in shape of these sun-spot, greatly favors the supposition that they are but openings in cloud-like matter; thus sun-spots have been observed, whose area was four times that of the entire surface of our globe, so that in fact four earths in line, arm in arm, might have marched through this breach, without more than *touching* the edge; or regarding this as a cavity, our globe might be at the bottom of it like a bean in a tea-cup. The edges of these spots are found to move at various rates, sometimes as great as 44 miles in an hour. All this is comprehensible in the case of cloud matter, but difficult to apprehend with any less mobile material.

Another class of resemblances between the action of sun-spots and of tornadoes, gives further support to the hypothesis stated above. Tornadoes, as it is well known, are confined chiefly to two zones, north and south of the equator, and similarly we find the sun-spots restricted by boundaries having like relative positions with reference to the solar equator. Now, we believe the development

of tornadoes to be controlled substantially by the excessive heat of the tropics and aerial motions resulting from this cause, combined with the rotary motion of the earth. The motions about the equators of the sun and earth are similar in character, and differ only in degree; the equatorial velocity of the sun being about four times that of the earth. Is there, then, any reason for supposing similarity as regards excess of heat upon the equator? If our hypothesis as to the origin of solar heat may be trusted, this condition is exactly what we should anticipate. The zodiacal light or cloud of infalling matter, lies sensibly in the ecliptic, whose plane makes, with that of the sun's equator, an angle of only 7°. Hence, it is upon the equatorial and not upon the polar regions of the sun that these heat-developing meteors must chiefly fall.

It is true that this does not explain the fact that the sun-spots do not occur upon the equator itself, but only in zones at either side, like those of our trade winds, but neither is this fact inconsistent with the above conclusion, but simply requires some further knowledge for its explanation.

(To be continued.)

LECTURES ON VENTILATION.

BY LEWIS W. LEEDS.

Second Course, delivered before the Franklin Institute, during the winter of 1867–68.

(Continued from page 65).

THE phenomena of a burning lamp or candle, is a beautiful emblem of human life. By way of illustrating this part of my subject, I have here a common coal-oil lamp with glass chimney attachment; having lighted which, I will paste a piece of paper over these openings at the base, intended as entrance-passages to the air. The evil result of this operation is visible at once; the flame looks bleared and it smokes most miserably; that lamp, to all appearances, has certainly got the dyspepsia!

The food that feeds that flame is simply undigested for the want of pure air—for the want of oxygen—in other words, for *lack of ventilation.*

Similarly, when you eat your full allowance of food, and do not

breathe sufficient pure air to warm and purify your blood, your whole system becomes *filled* with undigested carbon or "smoke," as we may call it, the same as with that lamp. Likewise the same amount of food will be of less than half its value: just as that oil gives less than half its full light.

One most excellent way to fill your system with smoke, and destroy half your usefulness, is as follows: get up late in the morning, eat a hurried breakfast, and immediately rush to the cars. Sit half or three-quarters of an hour in the close, foul car, and if your feet get thoroughly chilled, all the better for that purpose; a pair of tight boots is the proper thing to prevent the natural circulation of the blood. Have a care to sit with your back to the open window, and your face towards the centre of the foul car; that, too, helps. Your breakfast most probably may not have digested well, and if you feel pretty miserable in about two or three hours, you should commence eating ground-nuts, and keep on at irregular intervals until lunch-time. At this happy period, if you are still a "little under the weather," and not very hungry, go to the restaurant, and eat merely a piece or two of *home-made pie*, with a glass of milk, nothing more! and I should think that would be about sufficient to keep you up to the American standard of the shameful abuse of health. If there should be anything wanting, however, you might accept an invitation to a party, and after spending several hours in a hot, unventilated room, of course you would be too much fatigued to walk home—in fact your thin-soled shoes and light party-dress won't admit of it—and home you ride in a car conditioned as regards wholesomeness and temperature, as aforesaid.

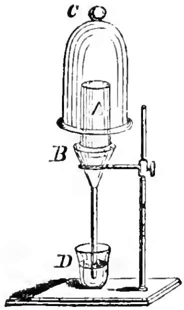
You may not see at once what connection these physiological explanations have with ventilation; but it is the severest tax upon my ingenuity, so to warm and ventilate a room or car, as fully to meet these imperative demands of the physiologist.

Let me say in a few words, always keep your feet warmer than your head, and your back warmer than your face. I will not say now, turn your back to your enemy, but I *will* say, never turn your back to *your best friend*. Eat heartily twice in twenty-four hours (never more than three times), of good, wholesome food, and always after eating sit down for a half or three-quarters of an hour. Remain perfectly quiet, in a room ten degrees hotter than the ordinary temperature of that room, keeping the feet warm, dry and free from pressure, and the back thoroughly protected from draughts,

with a cool fresh breeze blowing in your face. Follow this up carefully, for awhile, and you will not say you cannot afford the time to spend thus, but you will say you cannot afford to *omit* it.

We have here an arrangement by which we wish to explain in a manner the *diffusion of gases;* it has been furnished me by Prof. Albert Leeds, to whom I may say here, I have been much indebted for collateral information and assistance in these lectures.

Fig. 5.



It is often remarked that one gas is a vacuum for another gas. This is hardly the proper way of expressing it, because that would seem to imply that a cubic foot of carbonic acid gas would be diffused into a cubic foot of hydrogen gas, both occupying the one space with the original foot of the latter; but this is not the case. They become mixed, however, by exchanging places. For instance, if we take a glass vessel, just one foot square, containing, consequently, one cubic foot of hydrogen gas, which would weigh thirty-six grains, and place it over a similar vessel, containing one cubic foot of carbonic acid, which would weigh 815 grains, or more than twenty-two times as much as the hydrogen, the heavy carbonic acid gas would rise up into the vessel above, and an equal volume of the light hydrogen gas would fall into the vessel below, so that they would soon become equally diffused.

The apparatus which we have here for illustrating this beautiful phenomenon, consists of a glass tube, with its lower end dipping into a vessel of liquid (crimson-colored, so as to allow its movements to be readily seen), and over the top is placed and sealed in position, an unglazed clay jar. Now, into the glass bell-jar which I hold, mouth downwards, we will allow a supply of hydrogen gas to enter, which, as it is about fifteen times lighter than the air in this room, it will readily do, rising directly to the top, and displacing the air. The vessel being filled, as I judge, I will place it over the unglazed jar, and at once you see the air, as it is expelled from the bottom of the tube, bubbling up through the liquid in the reservoir. This is caused simply by the hydrogen gas rushing through that porous clay cylinder, and displacing an equal amount of the air.

But now we will reverse the operation. We will remove the glass vessel previously filled with hydrogen gas, and then, as the porous cylinder will contain a greater proportion of hydrogen than

the surrounding air, the excess of the gas will rush back again. This result you see palpably and beautifully demonstrated by the rising of the crimson fluid in the tube, and its coursing around the spirals.

Now, this law of the diffusion of gases, is a vitally important one. Did they not possess this property, the different gases would envelope the earth in distinct and separate belts or layers. The poisonous carbonic acid gas being formed in a great miasm near the ground, and being fifteen times heavier than atmospheric air, would cover, primarily, the surface of the earth; then would come a layer of oxygen, next a layer of nitrogen, and so on. Nor would heating them, even supposing the heat should be applied at the bottom, be sufficient to mix them thoroughly, because each density, it would be found, would circulate nearly horizontally by itself.

Hence, it is evident that this becomes an important question with reference to the subject of ventilation; for the reason that, in a still room, although the breath may at first fall to the floor, owing to its having a hundred times as much carbonic acid as ordinary air, yet that would soon be diffused through the whole room. And yet I do not pay so much attention to this, because I think the supply of air ought to be so rapid and abundant, that the slow process of the diffusion of gas would be permitted to exercise but little practical influence.

(To be continued.)

Franklin Institute.

Proceedings of the Stated Monthly Meeting, December 16th, 1868.

The meeting was called to order, with the Vice-President, Mr. Coleman Sellers, in the Chair.

The minutes of the last meeting were read and approved.

The Actuary submitted the minutes of the Board of Managers, and reported that at their stated meeting, held December, 9th instant, the resignations of membership in the Board by Messrs. Percival Roberts, William J. Horstmann, and Jacob G. Neafie were accepted.

Also, that donations to the Library were received from the Royal

Geographical Society, the Institute of Actuaries, the Society of Arts, and the Commissioners of Patents, London; the Smithsonian Institution, Washington, D. C.; William B. Thomas, Esq., Burlington, New Jersey, and Joel Giles, Esq., Philadelphia.

The various Standing Committees reported their minutes, and the Special Committee on a revision of the patent laws, reported progress. A paper on the Manufacture of Nitro-Glycerine, communicated by Mr. Stephen Chester, was then read by the Secretary, and the thanks of the Society presented to the author by the Vice-President for the same.

The regular report of the Resident Secretary on Novelties in Science and the Mechanic Arts, was then read, after which some interesting facts in connection with the raising of a portion of the city of Boston, and with the progress of the Suez Canal, were mentioned by Mr. Robert Briggs.

On motion, the Society then went into nomination of officers and members of the Board of Managers for the ensuing year, when the following nominations were made:

President—J. Vaughan Merrick.
Vice-President—B. H. Moore.
Treasurer—Frederick Fraley.
Secretary—Henry Morton.
Auditor—Samuel Mason.

BOARD OF MANAGERS.

John H. Towne,	Geo. P. Roberts,	H. A. Bines,
Washington Jones,	James S. Whitney,	R. H. Lang,
Pliny E. Chase,	James Dougherty,	Wm. B. Wilstack.
Chas. S. Close,	Robert C. Cornelius,	John Birkbeck,
Robert Briggs,	Caleb S. Hallowell,	Chas. Wheeler,
J. Hayes Linville,	Alexander Ervine,	Wm. Helm,
Joseph M. Wilson.		

The Chair then appointed as Judges of Election, Wm. A. Rollin, C. S. Bement, Samuel Hart, Hector Orr, M. W. Haines, Roeper Hoskins, Geo. Gardom.

Mr. Robert Briggs then offered the following resolutions:

Whereas, The rights of ownership of literary productions are based upon the same equitable considerations as those which exist in mechanics' inventions, and both classes of rights possess that individuality which should entitle the author or inventor to a means of protection from the law, well defined and of equal distinctness

to that which the law gives as security to personal property in chattels, or to property in real estate.

And whereas, The complete acknowlegment of the rights of ownership in literary productions, irrespective of nationality, is due to a sense of justice.

Therefore, resolved, That the Franklin Institute, as the representative of the interests of the scientific men, the manufacturers, inventors and mechanics of Philadelphia, expresses this opinion, that it is now desirable that such law, as will establish, define and protect the rights of authors should be enacted by our general Government.

Resolved, 2d. That negotiations and treaties should be formed to the end of obtaining for American authors the acknowledgment of such rights in foreign countries.

Resolved, 3d. That these Resolutions be transmitted to the Senators of our State in the U. S. Senate, to be laid before that body as an expression of opinion by the Institute.

That a Committee of five be appointed to solicit other literary and scientific associations to join in this memorial.

After a brief discussion, it was decided that the consideration of this subject was not within the scope of the Institute, and the motion was therefore laid on the table. The meeting was then on motion adjourned.

HENRY MORTON, *Secretary.*

Sea-Weed Charcoal.—This material, which is prepared from the fine tangle of the Hebrides, is being extensively used in England, as a substitute for animal charcoal, as a filtering medium for water, for deodorizing sewage, clearing white glass, removing acidity from and decolorizing wines and precipitating and decolorizing vegetable alkaloids.—*Chemical News*, p. 49.

A COMPARISON *of some of the Meteorological Phenomena of* DECEMBER, 1868, *with those of* DECEMBER, 1867, *and of the same month for* EIGHTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11¼′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	December, 1868.	December, 1867.	December, for 18 years.
Thermometer—Highest—degree	46·00°	52·50°	71·00°
" date	21st.	27th.	2d, '59.
Warmest day—mean	41·33	47·33	62·80
" " date	21st.	28th.	2d, '59.
Lowest—degree	13·00	8·00	4·50
" date	25th.	12th & 14th.	19th, '56.
Coldest day—mean	20·33	14·17	11·00
" " date	24th.	13th.	18th, '56.
Mean daily oscillation	9·18	11·84	11·93
" " range	4·22	6·16	6·35
Means at 7 A. M.	30·72	29·26	31·78
" 2 P. M.	34·56	34·13	38·64
" 9 P. M.	32·36	31·77	34·49
" for the month	32·55	31·72	34·97
Barometer—Highest—inches	30·489	30·558	30·678
" date	13th.	19th.	18th, 56.
Greatest mean daily pressure	30·415	30·516	30·611
" " " date	13th.	19th.	18th, '56.
Lowest—inches	29·398	29·548	28·946
" date	17th.	6th.	9th, '55.
Least mean daily pressure	29·520	29·704	29·175
" " " date	8th.	17th.	8th, '54.
Mean daily range	0·239	0·256	0·219
Means at 7 A. M	30·042	30·038	29·960
" 2 P. M	30·027	30·027	29·920
" 9 P. M	30·052	30·032	29·948
" for the month	30·040	30·032	29·943
Force of Vapor—Greatest—inches	0·240	0·361	0·551
" date	20th.	27th.	2d, '59.
Least—inches	·051	0·55	·025
" date	24th.	13th.	18th, '56.
Means at 7 A. M	·123	·133	·144
" 2 P. M	·124	·131	·162
" 9 P. M	·139	·146	·156
" for the month	·129	·137	·154
Relative Humidity—Greatest—per cent	90·0	93·0	100·0
" date	Often.	12th & 27th	Often.
Least—per cent	38·0	41·0	23·0
" date	10th & 19th	29th.	15th, '61.
Means at 7 A. M.	69·6 per ct	78·5 per ct	77·1 per ct
" 2 P. M.	60·2	65·1	64·9
" 9 P. M.	73·4	77·1	75·2
" for the month	67·7	73·6	72·4
Clouds—Number of clear days*	8·	5·	8·6
" cloudy days	23·	26·	22·4
Means of sky covered at 7 A. M	62·3 per ct	72·9 per ct	64·0 per ct
" " " 2 P. M	65·5	70·6	63·8
" " " 9 P. M	51·6	52·6	49·1
" " for the month	59·8	65·4	59·0
Rain and Melted Snow—inches	4·27	2·860	3·793
No. of days on which rain or snow fell	8·	·12	10·5
Prevailing Winds—Times in 1000	N54°13′W.327	S52°46′W.201	N62°46′W.272

* Sky one-third or less covered at the hours of observation

A COMPARISON *of some of the Meteorological Phenomena of the year* 1868, *with those of* 1867, *and of the last* SEVENTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11¼′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	1868.	1867.	17 years.
Thermometer—Highest—degree.........	98·00°	91·00°	101·00°
" date.............	July 14.	July 4.	July 17, '66.
Warmest day—mean....	90·50	85·67	92·33
" " date......	July 14.	July 4.	July 17, '66.
Lowest—degree..........	3·00	8·00	—9·00
" date.............	Feb. 23.	Dec. 12 & 14.	Jan. 8, '66.
Coldest day—mean.......	10·83	14·17	—1·00
" " date........	March 3.	Dec. 13.	Jan. 9, '56.
Mean daily oscillation...	13·35	14·48	14·71
" " range.........	5·29	5·30	5·52
Means at 7 A. M............	48·87	49·27	49·88
" 2 P. M............	57·31	58·17	59·67
" 9 P. M............	52·29	52·78	53·25
" for the year.......	52·82	53·41	54·27
Barometer—Highest—inches...............	30·748	30·970	30·970
" date..................	Feb. 1.	Feb. 11.	Feb. 11, '67.
Greatest mean daily pressure	30·672	30·862	30·862
" " " date...	Feb. 23.	Feb. 11.	Feb. 11, '67.
Lowest—inches................	29·115	28·778	28·778
" date..................	March 2.	May 8.	May 8, '67.
Least mean daily pressure...	29·249	29·013	28·958
" " " date.....	March 2.	May 8.	Ap'l 21, '52.
Mean daily range............	0·185	0·187	0·159
Means at 7 A. M................	30·028	29·996	29·892
" 2 P. M................	29.995	29·962	29·853
" 9 P. M................	30·012	29· 977	29·879
" for the year.............	30·012	29·978	29·875
Force of Vapor—Greatest—inches.......	0·911	0·925	1·059
" date	July 12.	July 6.	June 30, '55.
Least—inches..........	·038	·042	.013
" date	Feb. 23.	Jan. 30.	Feb. 6, '55.
Means at 7 A. M.......	·321	·316	·324
" 2 P. M.........	·347	·325	·339
" 9 P. M........	·349	·341	·346
" for the year......	·337	·327	·336
Relative Humidity—Greatest—per cent.	100·0	100·0	100·0
" date.....	March 21.	Oct. 29.	Often.
Least—per cent.......	28·0	21·0	13·0
" date........	Oct. 17.	April 6.	Ap'l 13, '52.
Means at 7 A. M.......	75·5	75·6	75·5
" 2 P. M.......	59·8	57·7	57·3
" 9 P. M......	73·5	71·9	72·1
" for the year......	69 6	68·4	68·3
Clouds—Number of clear days*.........	82·	108·	108·
" cloudy days..........	284·	257·	257·
Means of sky covered at 7 A. M.	66·6 p. c.	62·9 p. c.	60 5 p. c.
" " " 2 P. M.	63·8	61·9	61·1
" " " 9 P. M.	54·0	48·6	46·5
" " " for the year.....	61·5	57·8	56·0
Rain and melted snow—Amount—inches	50·180	62·935	47·117
No. of days on which rain or snow fell...	128·	134·	127·
Prevailing Winds—Times in 1000.......	N74°58′W.194	N71°34′W.152	N74°58′W.194

* Sky one-third or less covered at the hours of observation.

A General Abstract of the Meteorological Observations made at Philadelphia during the year 1868. By James A. Kirkpatrick, A.M.
Latitude 39° 57½′ N. Longitude 75° 11¼′ W. from Greenwich. Height of Barometer fount, sixty feet above mean tide in the Delaware River.

1868.	Thermometer.									Barometer.								Dew Point.			
			Range.			Means.						Range.		Means.				Means.			
MONTHS	Maximum.	Minimum.	Monthly.	Mean daily.	Mean daily oscillation.	7 A. M.	2 P. M.	9 P. M.	Average.	Highest.	Lowest.	Monthly.	Mean daily.	7 A. M.	2 P. M.	9 P. M.	Average.	7 A. M.	2 P. M.	9 P. M.	Average.
	°	°	°	°	°	°	°	°	°	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	°	°	°	°
January......	46	11	35	5·80	11·79	27·84	33·23	29·87	30·31	30·580	29·291	1 289	·271	30·010	29·986	30·028	30·008	22 64	22·21	23·88	22·91
February....	52	3	49	7·84	16·00	21·72	31·21	26·72	26·55	·748	·566	1·182	·278	30·197	30·154	30·156	30·169	16·58	21·53	21·28	19·80
March..	76	6	70	6·97	15·68	35·55	46·34	40·69	40·86	·600	·115	1·485	·258	30·041	30·021	30·032	30·031	28 54	31·73	32 28	30·85
April.........	70	23	47	7·73	16·80	42·28	52·78	46·38	47·15	·458	·192	1·266	·241	30·035	29·980	29·989	30·001	33·54	35·92	37·41	35·62
May..........	77	40	37	4·65	14·80	54.32	63·50	57·19	58·34	·209	·375	·834	·144	29·846	29 835	29·841	29·841	47·53	50·08	50·71	49·44
June.........	90	51	39	4·10	15·97	67·12	77·85	70·85	71·94	·292	·629	·663	·119	30·000	29·970	29·978	29·983	58·38	60·30	61.09	59·93
July..........	98	65	33	3·13	13·32	77·26	86·60	80·79	81·55	·163	·623	·540	·064	29·962	29·928	29·942	29·944	68·70	69·74	70·52	69·65
August.......	89	61	28	2·87	10·65	74·26	80·90	77·37	77·51	·279	·627	·652	·102	30·006	29·976	29·986	29·989	64·73	64·84	65·80	65·12
September...	88	48	40	5·70	11·83	64·10	72·18	67·37	67·88	·441	·764	·677	·123	30·046	29·997	30·021	30·022	55·64	57·25	58·03	56·97
October	70	34	36	5·12	12·06	48·92	58·24	52·92	53·36	·552	·685	·867	·190	30·144	30·105	30·122	30·124	41·84	43·47	43·90	43·07
November...	72	33	39	5·37	12·10	42·38	50·30	45·02	45·90	·418	·355	1·063	·187	30·005	29·963	29·992	29·987	34·99	33·26	35·74	34·66
December ...	46	13	33	4·22	9·18	30·72	34·56	32·36	32·55	·489	·398	1·091	·239	30·042	30·027	30 052	30·040	22.10	21·89	24·67	22·88
Annual means, } ...	98	3	95	5·29	13·35	48·87	57·31	52·29	52 82	30·748	29·115	1·633	·185	30·028	29·995	33·012	30·012	41·27	42·68	43·78	42·58
Winter......	52½	3	49½	6·60	13·31	26·27	32·86	29·45	29·53	30·748	29·291	1·457	·268	30·082	30·056	30·072	30·070	20 85	22·36	23·53	22·25
Spring........	77	6	71	6·45	15·76	44·05	54·21	48·09	48·78	·600	·115	1·485	·214	29·974	29 945	29·954	29·958	36·54	39·24	40·13	38·64
Summer......	98	51	47	3·37	13·31	72 88	81·78	76·34	77·00	·292	·623	·669	·095	29·989	29·958	29·969	29·972	63·94	64·96	65 80	64 90
Autumn.....	88	33	55	5·40	12·00	51·80	60·24	55·10	55·71	·552	·355	1·197	·167	30·065	30·022	30·045	30·044	44·16	44·66	45·89	44·90
Means for 17 years. }	101	—9	110	5·52	14·71	49·88	59·67	53·25	54·27	30·970	28·778	2·192	·159	29·892	29·853	29·879	29·875		43·26		

TABLE—(continued.) *A general abstract of the Meteorological Observations made at Philadelphia during the year 1868.*

1868. MONTHS.	Relative Humidity.							Force of Vapor.							Clouds. Sky Covered.				Rain or melted snow.		Winds.	
	Maximum.	Minimum.	Range.	Means. 7 A. M.	Means. 2 P. M.	Means. 9 P. M.	Average.	Maximum.	Minimum.	Range.	Means. 7 A. M.	Means. 2 P. M.	Means. 9 P. M.	Average	Means. 7 A. M.	Means. 2 P. M.	Means. 9 P. M.	Average	Amount.	No. of days it fell.	Monthly resultant.	No. of times in 1000.
	pr ct	pr ct	pr ct	pr ct.	pr ct.	pr ct.	pr ct.	inch.	inch	inch.	inch	inch	inch	inch	pr ct	pr ct	pr ct	pr ct	inch	days	Direction from	
January.....	95	30	65	80·6	64·7	77·8	74·4	·247	·048	·199	·129	·127	·135	·130	57·7	60·3	52·2	56·7	3·49	13	n. 79° 43′ w.	265
February....	95	42	53	80·0	67·1	79·5	75·6	·231	·038	·193	·100	·121	·121	·114	64·5	50·7	47·9	54·4	2·59	6	n. 77° 48′ w.	240
March........	100	30	70	76·3	58·9	72·6	69·3	·574	·042	·532	·168	·199	·200	·189	73·2	59·0	57·7	63·2	3·80	11	n. 17° 21′ w.	095
April.........	94	30	64	72·2	56·0	72·1	66·8	·528	·076	·452	·202	·236	·242	·227	61·0	65·7	57·3	61·3	4·89	13	s. 87° 43′ w.	153
May...........	97	30	67	78·7	64·1	79·7	74·2	·592	·177	·415	·338	·376	·379	·364	80·3	77·7	61·6	73·2	6·89	13	n. 21° 32′ e.	245
June..........	94	35	59	74·3	56·7	72·2	67·7	·815	·255	·560	·501	·540	·551	·531	62·0	66·0	51·3	59·8	3·69	11	n. 12° 32′ w.	058
July..........	90	39	51	75·3	58·3	71·6	68·4	·911	·507	·404	·705	·732	·752	·729	72·6	65·2	64·5	67·4	2·63	9	s. 15° 15′ w.	131
August.......	83	39	44	72·3	58·8	67·9	66·3	·843	·386	·457	·620	·625	·644	·630	66·1	68·7	46·1	60·3	2·55	10	s. 46° 44′ w.	271
September...	97	34	63	74·8	60·5	72·8	69·4	·846	·172	·674	·473	·503	·505	·494	73·0	68·7	59·0	66·9	8·61	14	s. 54° 3′ w.	082
October......	91	28	63	76·8	59·6	72·3	69·6	·534	·091	·440	·281	·304	·303	·296	65·2	62·9	54·5	60·9	2·24	11	n. 22° 18′ w.	241
November...	93	36	57	75·3	52·6	70·5	66·1	·486	·104	·382	·212	·201	·217	·210	61·7	55·0	44·0	53·5	4·53	9	n. 70° 21′ w.	258
December...	90	38	52	69·6	60·2	73·4	67·7	·240	·051	·189	·123	·124	·139	·129	62·3	65·5	51·6	59·8	4·27	8	n. 54° 13′ w.	327
Annual means.	100	28	72	75·5	59·8	73·5	69·6	·911	·038	·873	·321	·341	·349	·337	66·6	63·8	54·0	61·5	50·18	128	n. 70° 19′ w.	138
Winter.......	95	30	65	79·7	65·6	78·1	74·5	·361	·038	·323	·121	·126	·134	·127	65·0	60·5	50·9	58·8	8·94	31	n. 72° 3′ w.	235
Spring........	100	30	70	75·7	59·7	74·8	70·1	·592	·042	·550	·236	·270	·274	·260	71·5	67·5	58·8	65·9	15·58	37	n. 15° 35′ w.	108
Summer.....	94	35	59	74·0	57·9	70·6	67·5	·911	·255	·656	·609	·632	·649	·630	66·9	66·6	54·0	62·5	8·87	30	s. 43° 2′ w.	122
Autumn.....	97	28	69	75·6	57·6	71·9	68·4	·846	·094	·752	·322	·336	·342	·333	66·6	62·2	52·5	60·4	15·38	34	n. 56° 19′ w.	162
Means for 17 years.	100	13	87	75·5	57·3	72·1	68·3	1·059	·013	1·046	·324	·339	·346	·336	60·5	61·1	46·5	56·0	47·12	127	n. 74° 58′ w.	194

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EDITORIAL.

ITEMS AND NOVELTIES.

Fairmount Water Works—New Wheels and Pumps.—The new wheel just started at the Fairmount Works is of the kind called "Jonval's Turbine." It is 10 feet 3 inches in diameter; the movable wheel having 49 buckets 17 inches deep and 21 inches wide. The power is transmitted through a bevel pinion of iron 5 feet 6 inches diameter, 18 inches wide on the face, working into a bevel wheel with wooden cogs 8 feet diameter, 18 inches face: this drives an iron spur pinion 5 feet diameter, 26 inches face, working into a spur wheel with wooden cogs 10 feet diameter, 26 inches face, fixed upon the main shaft, upon the ends of which are two crank-wheels 8 feet diameter, from which the pumps are driven. The main shaft is of cast iron, 25 feet 6 inches long, 18 inches diameter; the jour-

nals being 26 inches long, 15 inches diameter. The vertical shaft of the turbine is 10 inches diameter, 16 feet long, at the bottom of which is the stop upon which the wheel rests: this is made of lignum-vitæ, with the grain up; it is 17 inches diameter, and is kept constantly lubricated by a small stream of water from the reservoir.

The wheel is enclosed in a water-tight cast iron case, 12 feet diameter, and is provided with the usual draft-box and starting-gate common to wheels of this description.

The tail-race is provided with a drop-gate, so that it may be closed by raising this gate; the wheel-pit will then be pumped out by means of a centrifugal pump, driven by a small turbine 9 inches in diameter, supplied with water from ascending main, and will, therefore, have a head upon it of about 96 feet. By this arrangement, access can be had to the stop and gates of the wheel, should accident occur to them.

One disadvantage the wheel has to contend against is the variable head and fall under which it is required to work, ranging from 6 to 13 feet, as the tide rises or falls; this prevents the wheel giving as economical a result as it would with a constant head, as the wheel must be able to exercise its whole power at the minimum; it of course has too much power at the maximum head.

The wheel drives two double-acting force pumps of 22 inches diameter and 6 feet stroke; they are placed perfectly horizontal upon a continuous cast iron box bed-plate, which rests upon a granite foundation, the whole being secured to the solid rock by 2-inch bolts.

The water is taken into the pumps from the flume through two valve-chests, containing six brass clack-valves on each seat, placed at an angle of 45 degrees; these valves are of different sizes, the top ones having a clear opening of 5 by 16 inches, the next $5\frac{1}{2}$ by 16 inches, and the lower ones 6 by 16 inches; by this arrangement the valves do not close at precisely the same time, thus avoiding much of the shock usual upon pump-valves; the discharge-valves, are balanced tripple-beat cornish-valves.

The pumps are supplied from a wrought-iron flume, which also conducts the water upon the wheel; the inlet openings to the pumps are provided with cast iron gates, and the ascending main with a stop-cock; by shutting these, and disconnecting the connecting rod, either of the pumps may be thrown out of use, thus avoiding the

disadvantage of disusing both pumps when only one may be out of order.

The discharge-valves have air-chambers over them, 42 inches inside diameter, 7 feet high; the discharge-pipe from these two chambers are connected in a central air-chamber, 4 feet inside diameter, 10 feet high, from which passes a curved ascending main for each pump of 23 inches diameter, uniting by a breeches pipe in the ascending main proper, which is 36 inches diameter.

The pistons of the pumps are not packed in the ordinary way, but consist of cast iron cylinders, made as light as possible, somewhat like a broad driving pulley, the surface having a number of **V**-shaped grooves turned in it.

At the forebay end of the wrought iron flume are three cast iron sluice-gates, 5 feet high and 7 feet wide each, raised by screws and worm-wheel. The flume is of $\frac{1}{4}$-inch wrought-iron, with butt joints united by strong angle-iron ribs; it is 15 feet wide, 8 feet deep, and 24 feet long.

The speed of the pumps is intended to be 13 revolutions per minute; they will then have a capacity to raise 8,631,360 United States standard gallons per 24 hours.

The two breast-wheels taken out to make room for the turbine, could only raise about 2,836,080 gallons. The new wheel will, therefore, raise three times the quantity, and with a consumption of something less than 10 per cent. more water.

The ascending main, after it leaves the mill-house, passes over the forebay at a height of 9 feet above the level of the dam; it is of flanged pipes, 36 inches diameter, suspended by wrought-iron suspensions bars, 10 inches wide, $1\frac{1}{2}$ inches thick. These are attached by bolts and lugs to the top of the end pipes; the main thus forms its own bridge, the pipe itself being the top chord.

The clear span is 77 feet 11 inches; the total deflecture of the main when filled with water, and in use, is found to be $\frac{7}{16}$ths of an inch. After crossing the forebay, the main rises into the reservoir at an angle of about 43 degrees. Its total length from the pump to its discharge in the reservoir is 246 feet. Upon the upper end, which enters the reservoir a few feet below the ordinary surface of the water, is placed two clack-valves, at an angle of 45 degrees, in order to prevent the return of the water, should accident occur to the main.

The designs for the whole arrangement of the work—the pumps,

flumes, head-gates, the alteration of the mill-house, and the suspended main across the forebay—were made by the Chief-Engineer of the Water Department, Frederick Graff.

The design for the wheel and its gearing are by Emile Geylin, the contractor for the construction of the whole work. Mr. G. was the first to practically introduce the Journal Turbine into this country, and has been eminently successful with all the numerous wheels he has erected. The sub-contractors under Mr. G. were, for the pumps, bed-plates and connecting-rods, I. P. Morris & Co.; for the large wheel-case, the gearing, with its shafts and crank-wheels, the West Engine Co., of Norristown; the head-gates and the wrought-iron flume were made by Hunsworth & Naylor; and the turbine, and its fixtures, at the shop of Mr. Geylin.

Working of Steep Gradients.—An interesting paper on the Mauritius Railways was read before the Institution of Civil Engineers on the 2d of February, by Mr. J. R. Mosse, C. E., in which a full detail was given of the structure and working of the two railways which had been constructed in this locality.

The two lines were named, respectively, the North Line and the Midland Line.

The character of the gradients on the Midland Line will be understood from the following summary: From Port Louis to the summit there was a rise of 1817 feet in a distance of about sixteen miles, making an average gradient of 1 in 46·68. From the summit to Mahebourgh, a distance of about nineteen miles, the rate of descent was 1 in 55·61. For about twelve and a half miles before reaching the summit, the rise was 1 in 41·17, and from the summit, for about thirteen and a half miles, the fall was about 1 in 45·06.

The steepest gradient was 1 in 27, of which there was a total length of 13,526 feet, the greatest continuous length being 6163 feet, and the next 5016 feet. The next in severity was 1 in 30, of which there was altogether 9526 feet, the greatest continuous length being 3000 feet. The curves varied from 950 feet to 6000 feet radius, and in length from 200 to 3200 feet, the ordinary radii being 2000 and 3000 feet. The shortest radius occurring in the sharp gradients of 1 in 27 and 1 in 30, was 1600 feet, of which the greatest continuous length in the former was of 1930 and in the latter of 900 feet. The next in severity was with a radius of 2000 feet on the gradient of 1 in 27, the greatest continuous length of this being 1000 feet. Reverse curves of this radius were also found,

1920 feet in length, on the same gradient. On descending from the summit to Mahebourgh, the distance between the seventeenth and nineteenth mile might be considered as composed entirely of reversed curves.

The locomotives first used on this line, seven in number, had cylinders 16 inches in diameter, 22 inches stroke; the wheels, six in number, were 3 feet 6 inches diameter, and were all coupled, giving a wheel base of 15 feet. The weight of these engines, with fuel and water, was about 37 tons, and they were worked with 120 pounds steam pressure. Subsequently, six larger engines were designed by Mr. Hawkshaw, having the following dimensions: Cylinder, 18 inches in diameter, with stroke of 24 inches. The wheels, eight in number, were 4 feet in diameter, and all coupled, giving a wheel base of 15 feet 6 inches. The centre pair of wheels, which were the drivers, were fixed rigidly on the frame, but the leading and trailing wheels had $3\frac{3}{4}$ inches play in their journals, and the connecting rods between these and the driving pair were fitted with ball and socket joints.

These engines, with fuel and water, weighed 48 tons each. These engines took five passenger cars and one break van up the inclines, making a load of 42 tons, and sometimes even eight cars, equal to 56 tons, while of goods, the usual load for the smaller locomotives was 70, and for the larger 100 to 120 tons.

Average speed for passenger trains, including stops, was 12 miles per hour; for goods, 9 miles per hour on the up grades.

On one trip, where everything was carefully noted, the results were as follows: Ten loaded cars, weighing, together, 83 tons, were drawn, which, with the engine, made a gross load of 131 tons. The average speed was eleven miles per hour, and, taking the average pressure of steam in the cylinders as 60 pounds per square inch, the power exerted would be $\frac{131 \times 11 \times 88}{33 \cdot 000} = 230$ H. P., or, not including the weight of the engine, 146 H. P., and this divided by the weight of the locomotive, was 3·04 H. P. per ton of motor. The coal used was from Sidney, N. S. W., and the consumption amounted to 4·75 pounds per H. P. per hour, eight pounds of water being evaporated by one pound of coal, but ordinarily only seven and a half pounds of water were evaporated by each pound of coal.

All the carriages but the first-class ones had a break on each

wheel worked from inside. Clark's continuous break, which operates all the breaks in the train at once, has been used with great success, and each break was provided with sand boxes, leading to the rails; as before, the dew on the rails prevented their proper action. Fortunately, no accidents had occurred from trains getting beyond control on the heavy grades, although some narrow escapes had been made. Thus, on one occasion, the rails being wet, a train descended from the summit, a distance of three miles and three-quarters, in five minutes.

The working expenses thus far had been 62½ per cent. of the gross receipts, owing to exceptional conditions of irregular traffic, &c.

As a conclusion drawn from three years' experience with this road, the author of this paper had concluded that, although it might sometimes be impossible to construct a railway with easier gradients than those of this road, yet the difficulty of working in wet weather, the small loads carried, and high speed attained, rendered such inclines undesirable, even though a very great outlay was required at first to avoid them. In fact, the severest grade which should be adopted was 1 in 40. It was also suggested that in laying out such inclines, whatever might be the ruling gradient should be adhered to, as far as practicable, throughout, and that pieces of level should be introduced between the different inclines, as they were of the greatest value in controlling trains in descending.

Suez Canal.—The following figures show the condition of the work on the canal on 1st January, 1869: also the progress made during the past year. The two exhibits taken together, may give us the data for calculating the time when the entire work will be completed.

The estimates of quantities are given in *cubic metres*, to which 37 per cent. should be added to show the results in *cubic yards*.

The aggregate amount of earth to be moved, to dig the canal according to the plans adopted, was 74,112,130 cubic metres; of this there remained on 1st January, 1868, 40,000,000 cubic metres yet to be done.

During the past year the progress has been as follows:

First quarter	4,507,956
Second quarter	5,519,808
Third quarter	6,234,013
Fourth quarter	6,889,669
	23,151,446 cubic metres.

Leaving less than 17,000,000 cubic metres to be moved on the 1st January, 1869. The time now named by Mons. Lavalley for the entire completion of the work is 1st October next, and there seems to be no reason to doubt his ability to make good this prediction.

The success of the dredging machines has been even beyond the anticipations of their strongest advocates. One machine is credited with 108,000 cubic metres of excavation, in a single month; another with 88,889; another with 78,056 cubic metres within a like period. They have double gangs of men, and work night and day. Six dredges in November, in the Port Said division of the canal, raised 313,628 cubic metres; three other machines, at Ras-el-Ech, raised 214,042 cubic metres. The last new dredge of the contractors was put at work in December; and now their entire force, 60 machines, is being driven to its utmost capacity, in order that the canal in its full dimensions may be opened to the commerce of the world with the least possible delay. The piers or jettys at Port Said are entirely finished. The western pier was completed on 8th September, and the making of the concrete blocks was stopped the same day. On 15th December there remained but 316 blocks to be sunk to finish the eastern pier; and these could easily be handled in ten days.

The harbor and basins at Port Said have been dredged to a depth throughout of 23 feet; and now the French, Russian, Austrian and Egyptian steamers touch there regularly. No difficulty is experienced in running into this harbor at any time of day, or in any weather; whereas, at Alexandria, no vessel drawing 15 feet ever attempts to enter except by day light: and in heavy weather, steamers have been obliged to wait outside the bar for two and three days, on account of the narrow, shallow entrance to the harbor.

During the first six months of last year 513 vessels entered at Port Said, landing 3282 passengers, and 105,832 tons of merchandise. The viceroy of Egypt has ordered the line of railway between Cairo and Suez to be abandoned; and a new line of railroad has been constructed from Alexandria and Cairo to Suez, by way of Zagazig and Ismailia. This new route was opened in November last; and henceforth, Ismailia will be the stopping place on the Isthmus for passengers between Europe and India, while waiting for their steamers either in the Red Sea or the Mediterranean.

Adjustable Hanger, by James W. Loraine.—At the last

meeting of the Franklin Institute there was exhibited the hanger shown in Figs. 1, 2 and 3, manufactured by Wood, Loraine & Co., of this city.

Fig. 1.

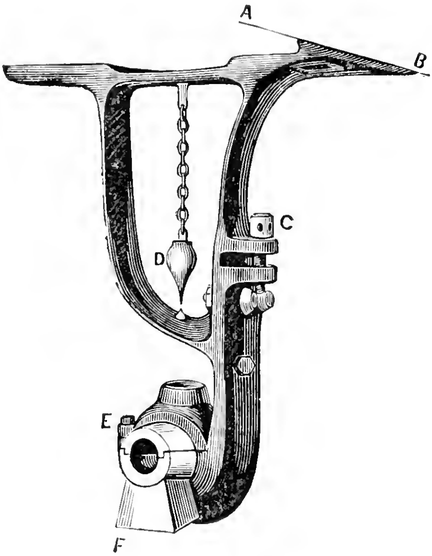


Figure 1 shows the apparatus entire, while Figures 2 and 3 show the upper and lower portions, respectively, detached from each other.

The straight edge, A B, is made exactly parallel with the axis of the journal, and the point of suspension and "sight point" of the plumb line, D, are exactly at right angles with this line, and in line with the centre of the journal.

This facilitates the setting of these bearings, as by drawing a line on the ceiling or girders parallel to the proposed line of shafting, setting the straight edge, A B, upon this, and then making the frame plumb by its own plumb-line, several of the

Fig. 2.

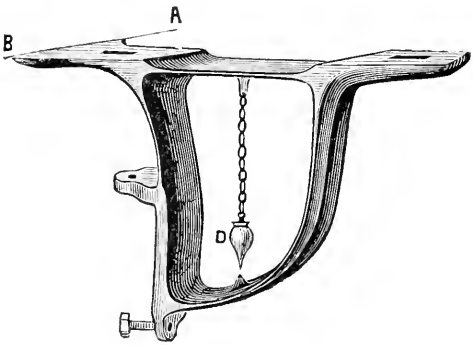
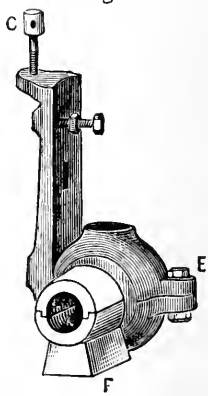
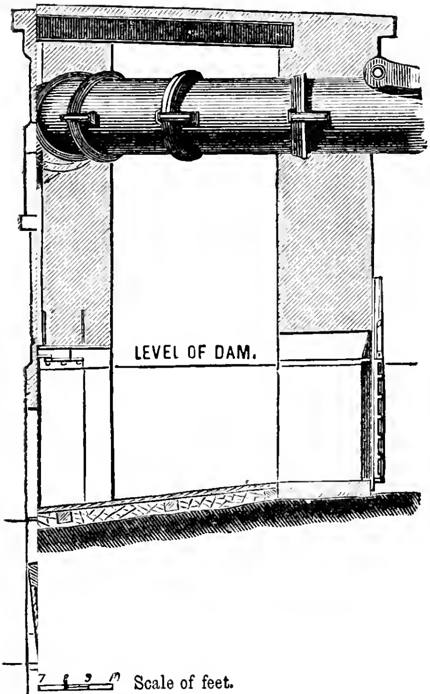


Fig. 3.

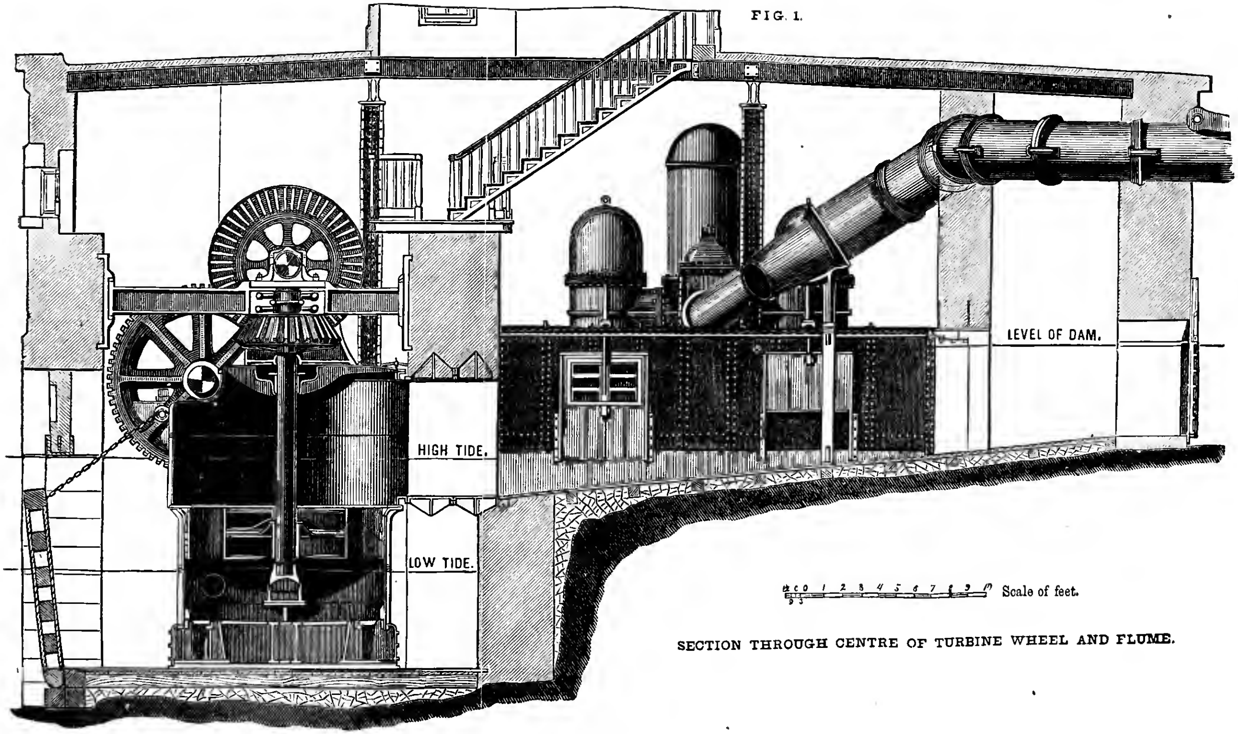


inconveniences attending the adjustment of ordinary hangers are avoided. Thus the stretched line for the centre of the shaft is dispensed with, as well as the various inconveniences accompanying its use.



TURBINE WHEEL AND FLUME.

FIG. 1.



SECTION THROUGH CENTRE OF TURBINE WHEEL AND FLUME.

After these upper parts are all in place, the lower portions are attached, the necessary vertical adjustment being obtained by means of the bolts passing through vertical slots shown in the cuts.

A great advantage is claimed for this hanger in case of erection, since the greater part of the weight is not attached until the setting of the upper portion is finished; and also because the entire enclosing of the socket gives it special advantage where exposed to severe side strains. The plumb and index point being always attached and in sight, show at once any deflection from line in their direction, when such occurs.

The Mont Cenis Tunnel.—During the past year an advancement of 1,320·15 metres has been made at the Mont Cenis tunnel, of which 638·60 was driven on the Italian side, at Bardonnêche, and 681·55 metres on the French, at Modane.

The following shows the monthly advancement, in metres, made during 1868:—

	Progress made at		Total advancement made during month.
	Bardonneche	Modane.	
January	54·30	51·90	106·20
February	49·00	47·05	96·05
March	49·30	60·85	110·15
April	46·90	62·45	109·35
May	61·30	54.50	115·80
June	59·80	54·15	113·95
July	63 90	64·80	128·70
August	52·10	56·80	108·90
September	50·00	56·85	106·85
October	52·20	63·25	115·45
November	56·10	61·85	117·95
December	43·70	47·10	90·80
Lengths driven during 1868	638·60	681·55	1,320·15
Lengths driven previous to 1868	4,724·50	3,122·15	7,846·65
Total lengths driven	5,363·10	3,803·70	9,167·80
Remaining to be driven			3,053·20
Total length of tunnel			12·220

This gives an average advancement of 110 metres per month, or 53·20 on the Italian side, and 56·80 on the French; and at this rate

of progress the time necessary for the completion of the tunnel would be 28 months, or about April, 1871, and for opening the railway about six months more, or in less than three years from the present time.

The following Table shows the yearly progress that has been made with these works since their commencement in 1857:—

YEAR.	Bardonneche	Modane.	Total Advancement.		Expenditure.
			Each Year.	At end of Year.	
	metres.	metres.	metres.	metres.	francs.
1857 1858	284 85	212 75	497·60	497·60	3,369,000
1859	236·35	132·75	369·10	866·70	1,630,000
1860	203·80	139·50	343·30	1,210·00	3,000,000
1861	170·00	193·00	363·00	1,573·00	2,500,000
1862	380·00	243·00	623·00	2,196·00	2,000,000
1863	426·00	376·00	802·00	2,998·00	3,500,000
1864	621·20	466·65	1,087·85	4,085·85	6,552,000
1865	765 30	458·40	1,223·70	5,309·55	5,502,000
1866	812·70	212·29	1,024·99	6,334·54	5,644,000
1867	824·30	687·81	1,512·11	7,846·65	6,000,000
1868	638·60	681·55	1,320·15	9,166·80	7,500,000
	5,363·10	3,803·70	9,166·80	....	47,197,000

Ellershausen Iron.—Much interest has been of late excited on the subject of a new process for the manufacture of wrought from cast iron, invented by Mr. Francis Ellershausen, of Pittsburgh, Pa. The process consists, essentially, in mingling together streams of melted cast iron and pulverized iron ore as they run into moulds, and then heating the conglomerate in a reverberatory furnace to a welding temperature, when it may be taken out, squeezed and forged, without other working in the furnace, and so produces iron of an excellent quality, with a greatly reduced cost of labor and fuel.

The exact method of procedure is as follows:

On the casting floor of the smelting furnace, a cast iron turn-table about 18 feet in diameter is revolved on rollers by a small steam engine. Upon the outside edge of the table stand a row of cast iron partitions, forming boxes, say 24 inches wide and 10 nches high, open at the top. Just above the circle of boxes stands

a stationary, wide-mouthed spout, terminating in the tap hole of the furnace. When the furnace is tapped, the liquid iron runs down this spout and falls out of it in a thin stream into the boxes as they slowly revolve under it, depositing in each a film of iron, say one-eighth of an inch thick. But before the fall of melted iron reaches the boxes it is intercepted, or rather crossed at right angles by a thin fall of pulverized iron ore, which runs out of a wide spout from a reservoir above. These two streams or falls are of about equal volume.

The thin layers of iron and ore at once chill and solidify, so that by taking out the outer partition of the boxes (which form the rim of the turn-table,) they may be removed in cakes of the size of the boxes, and weighing about one hundred pounds each. These cakes or blooms are put into a reverberatory puddling or heating furnace, and raised to a bright yellow heat. They will not melt at this heat, but become softened so as to be easily broken up with a bar. The blooms are formed in the furnace, by the "rabble" of the workmen, as in ordinary balling operations. The balls are brought out, one after another, squeezed in the ordinary "squeezers" to expel the cinder and superfluous ore, and then rolled into wrought iron bars, which are now ready for market, or for further reduction into smaller finished forms.

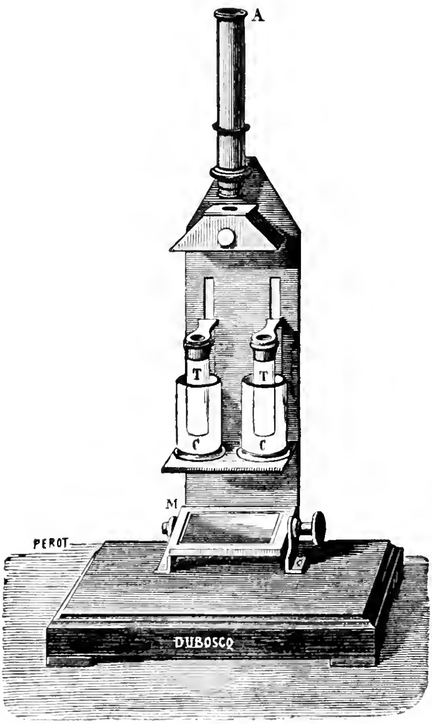
The essential features claimed are: First, that a conglomerate of iron and oxide such as is produced by the first step in the process, will not melt when submitted to the heat of a puddling furnace. Secondly, that the reaction which takes place between the intimately mingled iron and oxide in this last condition, suffices to remove impurities from the substance in such a way as to develop a first-class iron from good material, and as good a product as can be obtained by the ordinary process of puddling from inferior grades of ore and pig. The local papers speak in the highest terms of this process, and time alone is needed to settle the question of its actual value. The connection of this process with the theory lately advanced by Mr. Siemans, regarding the part played by the fettling in ordinary puddling, is worthy of notice. See this *Journal*, Vol. LVI, p. 252.

The patent by which this process is covered specifies as follows. "That the novelty or invention claimed consists in the mixing of solid oxides into and among fluid cast iron, or of fluid oxides with solid cast iron, granulated or minutely subdivided, in such a manner and in such quantity as to produce a solid conglomerate of the

two substances, and also in effecting this mixture, and producing the resulting pig bloom or pig scrap, without the application of other heat than that of the fused cast iron or oxide, as the case may be, thus dispensing with the use of a furnace for any part of the process of mixing after the melting of the cast iron or oxide, whichever of them is used in a fused condition.

The material thus produced may be used in like manner as any wrought iron of similar shape, so that when raised to a welding heat, the pig bloom, manufactured as hereinbefore described, may be pressed, squeezed, hammered, rolled, or worked in any of the methods employed in the treatment of wrought iron, and with like results, excepting that the article of wrought iron produced by this process is superior in quality to that obtained in the ordinary way.

Duboscq's New Colorimeter.—M. Duboscq has submitted to the Academy of Sciences his new colorimeter for measuring the differences of tint in solutions. The following description we find in Les Mondes. The two liquids are placed in the two cylindrical vessels, G G′, of glass, fixed side by side, before the vertical shelf of the colorimeter.



In the two vessels, two tubes of smaller diameter, T T′, closed at the lower extremity by a disc of glass, may be raised and lowered by means of the movable pinions engaged by two racks cut into the vertical table. To each pinion is fastened a vernier, which is moved under a graduated scale, and which measures the distance between the bottom of the vessels and the lower disc of the movable tube.

The luminous rays transmitted by the two columns properly illuminated by a mirror, M, placed above, and moved

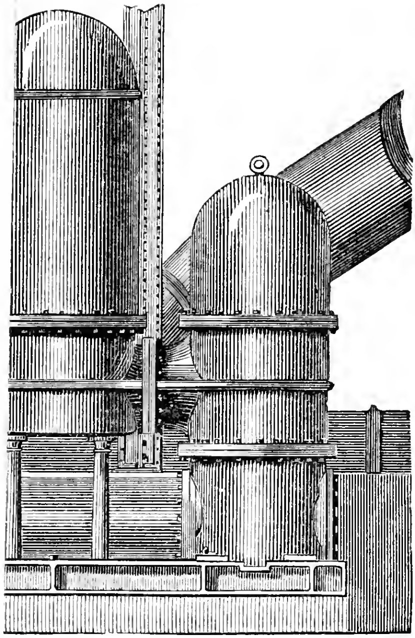


FIG. 6.

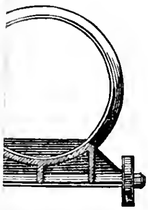
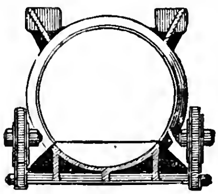


FIG. 5.



SECTION OF SUSPENDED MAIN.

SIDE ELEVATION OF FORCE-PUMP.

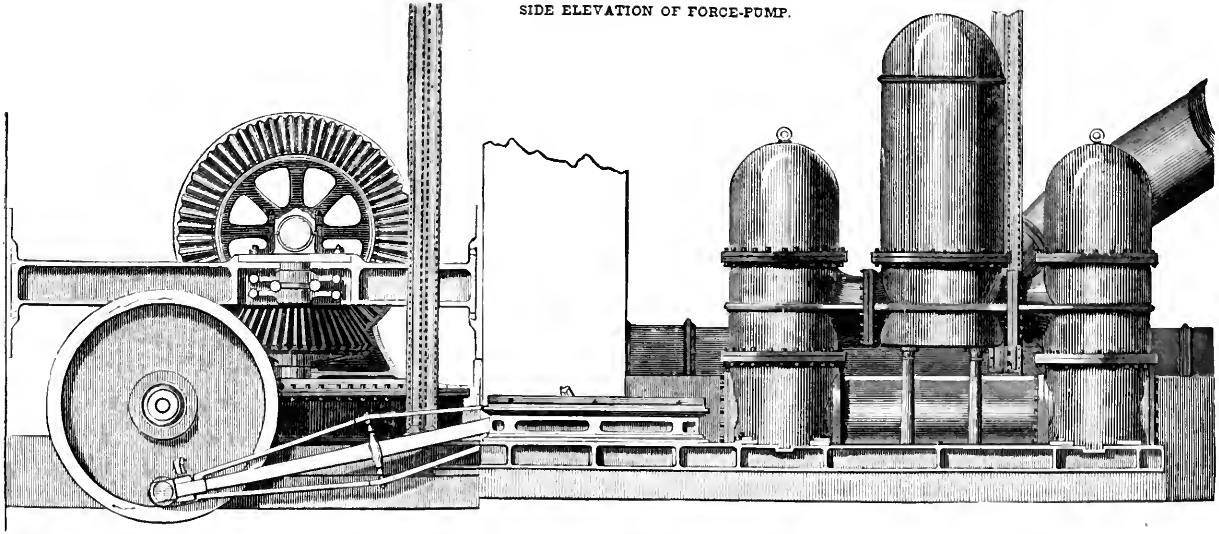
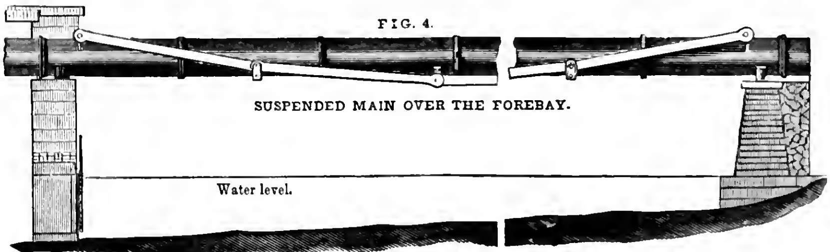


FIG. 4.



SUSPENDED MAIN OVER THE FOREBAY.

FIG. 6.

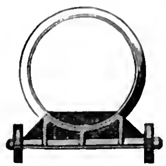
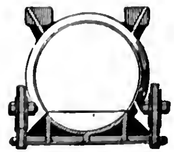


FIG. 5.

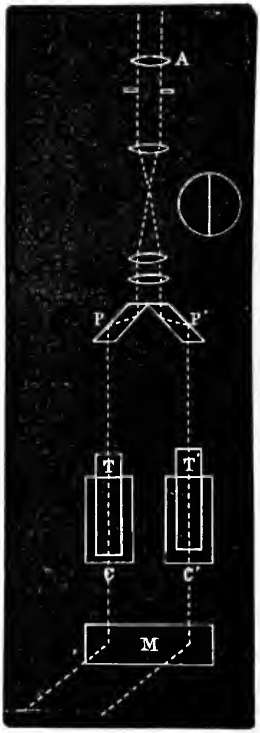


SECTION OF SUSPENDED MAIN.

around in a horizontal axis, suffer each two reflections, within one of Freswel's rhombs, P P′, and then arrive in the same field of vision, in such a manner that each shall illuminate the half of the field with a semi-disc or circle of yellow color, more or less intense. These colors are observed with a small lens, which is nothing but the base of a terrestrial eye-piece, formed of four glasses, and which magnifies sufficiently, so that the field may be illuminated by the colored plates with perfect uniformity.

The colors are proportional to the height of the columns if the liquid contain the same proportion of caramel; or proportional to the richness of the liquid in caramel, if the two columns have the same height.

In the last case, if we cause the height to vary, we have the same shade on these two semi-discs.



Suppose that the standard solution is in the cup to the right, and that we lower the interior disc or movable tube to 20 millimetres from the bottom of the cup; we will thus have a column of 20 millimetres, which gives to the half disc of the left hand side a pale yellow coloration. Admitting, then, that the half disc at the right has the same coloration, but that the column of the liquid placed in the cup to the left, has not a height of 40 millimetres, this informs us that the solution to be tried contains a proportion of caramel more than that contained in the standard liquid.

In fact, for the same color on the two semi-discs, the proportions of caramel contained in the two liquids are inversely as the height which we give to the column of the liquids.

One hundred centimetres of the solution tried, contained, then, in the case we speak of, $\frac{0 \text{ gr. } 2}{2}$ of caramel.

If we have dissolved, for example, 10 grammes of syrup in 100

cubic centimetres of water, we should know that 10 grammes of the syrup contained one d. c. g. of caramel.

Fall of a Chimney Stack.—Early on Sunday morning a chimney stack, 100 feet high, connected with the paper mills at Westend, Glasgow, fell during the gale. Near the base of the stack were a row of cottages, the roofs of two of which were crushed in, and the sleeping inmates buried in the ruins. Seven persons were killed on the spot, and another died in the course of the afternoon. *Engineering, Feb.* 19.

The Metal Hydrogen.—At a late meeting of the Royal Society, as we learn from the *Athenæum* of January 16th, Graham presented a specimen of Palladium, charged with some 800 or 900 times its volume of hydrogen, by some process which is not described in the above journal, but which, from his previous researches (see this *Journal*, Vol. LIV., p. 16, and Vol. LVI., p. 79), we presume consisted in heating it in an atmosphere of compressed gas. This specimen was accompanied by a paper, in which it was explained that the variations of density, of conducting power, &c., produced in the Palladium by the absorption of the hydrogen, seemed to indicate that a true alloy had here been formed, and thus to establish the metallic character of the consolidated gas. Various rumors of this circumstance have been circulating in our daily papers, in which the specimen presented by Mr. Graham was exalted into "an ingot of hydrogen," and though in comparison with this, the actual fact may seem disappointing, yet in its true relations it is sufficiently wonderful, and is certainly a decided step towards the not impossible realization of the veritable "ingot" at some future time. As a mere evidence of the intensity of molecular force, this experiment of Graham reaches into the marvellous and the incomprehensible. If the space occupied by the condensed hydrogen had been entirely void of all other matter, the force required to reduce 800 volumes to 1 volume would have been 800 atmospheres, or 12,000,000 pounds to the square inch, but with a metal like Palladium, as dense as lead, it would be a large allowance to suppose that $\frac{1}{1000}$th part of its volume were void space, or consisted of the instertices between its particles. To compress the eight hundred volumes into this bulk would then demand a force of *twelve million pounds*, or *six thousand tons per square inch.* Yet this inconceivable force is quietly exerted by the atoms of Palladium in their attraction for those of the hydrogen.

This substance, hydrogen, has other evidence of its metallic character beside these experiments of Graham. We do not allude to

its chemical and electrical connections with the metals, but to an action closely related to this absorption by Palladium, which, though for some time known, presents a new aspect when viewed in the light of this result.

In 1863, Dr. Charles M. Wetherill made a series of investigations on the Ammoniacal Amalgam, which very clearly demonstrated that the peculiar compound known by that name was not an alloy of any such compound as NH_4 with mercury, but was, in truth, a "suds" of mercury, frothed up with minute bubbles of hydrogen and ammonia, but yet holding the gas in such close union as evidenced a decided affinity between the two bodies. (*Silliman's Journal*, Vol. XL, p. 160). We might then justly consider this attraction for and retention of the hydrogen by the mercury, as being analogous to the infinitely more energetic action which is shown by Palladium, and like it, also, as indicating a tendency in the hydrogen to alloy itself in the manner of a metal with other metallic elements. Remarkable as is this element in its chemical relations, it is equally notable in another respect, about which a few words may be appropropriate (in connection with the late astronomical discoveries of which we have recently spoken), under the head of

The Cosmical Relations of Hydrogen.—When Miller and Huggins attacked, with the Spectroscope, the problem of the constitution of the nebulæ, which had successfully defied the most diligent telescopic research, and had demonstrated that many of these were of gaseous consistency, hydrogen was one of the substances first recognized in the wonderful nebula of Orion and in several others. Now, this nebula of Orion was believed by Lord Ross to have been completely resolved by his telescope into separate points of light,* and we should thus be led to conclude that it is, in fact, a vast system—an universe—of suns or luminous centres, none of them solid, however, but all, on the contrary, vast spheres of glowing gas, that gas being chiefly hydrogen, mixed with nitrogen.

When, in May, 1866, a star in the constellation of the northern crown suddenly burst forth with unprecedented splendor, and when examined with the spectroscope showed a spectrum such as had never before been encountered, consisting of such an one as our sun or an ordinary star gives, but with four bright lines, due to a gas-

* He says (*Cosmos*, Vol. IV., p. 306, Bohn's edition): "I think I may safely say there can be little if any doubt of the resolvability of this nebula. * * We could plainly see that all about the trapzeium was a mass of stars."

eous source of light, superposed, it was found that two of these lines (and those the most brilliant) were such as come from light emitted by intensely heated hydrogen. The natural conclusion from this was, that some half extinguished star or sun had been encountered by one of these nebulous masses of hydrogen, or else by some vast globe or planetary cloud of the same gas, which had lost its heat and ceased to be luminous. This true "planet" or "wandering sphere" of hydrogen, coming within range of the star's attraction, was drawn down to it, and by the arrest of motion and compression consequent upon its encounter, was itself heated to incandescence, and heated also the surface of the dead sun to a temporary but intense brightness.

Here, then, was presented the spectacle of a world on fire, in which the agent of destruction, or reconstruction, whichever it might be, was one of these celestial masses of hydrogen gas.

It is curious to reflect in this relation that, making due allowance for the probable distance of this star and the velocity of light, this sphere had been at rest for some ten or twelve years after its fiery ordeal, at the time when we witnessed the event as in actual progress.

When, about a year since, Graham subjected pieces of meteoric iron to the same treatment which had, in the case of ordinary iron, eliminated the carbonic oxide which it had absorbed while undergoing fusion in the smelting furnace, it was hydrogen gas in large quantity which was evolved, thus proving that in the furnace in which these "falling stars" were fused and cast into shape, this same widely distributed element was again predominant.

Lastly, in those spectroscopic observations and discoveries in connection with the sun, which we described in our last number, it seems to be very clearly shown that hydrogen gas is again the main constituent of that, which Lockyer proposes to call the solar "chromosphere," which surrounds the entire mass of our luminary for a depth of some five thousand miles, and forms those flames or protuberances, a single tongue of which, as in the last eclipse, may contain some 7,000,000,000,000 cubic miles, or twenty-seven times the Earth's volume of this gas.

We have reason, therefore, to wish that our knowledge of these solar appendages may not become *too* intimate, and that none of them may, by an excursion to this distance, furnish to other planets, at our expense, a second display of the phenomena exhibited in τ Coronæ Borealis.

Civil and Mechanical Engineering.

BELTING FACTS AND FIGURES NO. II.

BY J. H. COOPER.

(Continued from page 92.)

Superiority of the Driving Belt.

"There is no simpler or smoother means of communicating motion than that afforded by the noiseless agency of cords, bands or straps. The very means by which the motion is maintained, namely, by the frictional adhesion between the surfaces of the belt and the pulley is a safe-guard to the whole mechanism, as, if any unusual or accidental obstruction should intervene, the belt merely slips, and breakage and accident are thus prevented."—*Lond. Mech. Mag.*, Mar. 1863.

"The facility with which this communication of rotary motion may be established or broken at any distance, and under almost every variety of circumstance has brought the band so extensively into use in machinery, that it may be considered as one of the principal channels through which work is made to flow."—*Moseley.*

Care of Belts.

In order to have belts run well, they should be perfectly straight and be of equal thickness throughout their length, have but one laced joint; but if circumstances require any belts to be composed of several pieces, the ends should be evenly beveled, and united by one or other of the permanent ways already mentioned. The ends to be laced should be cut at right angles with the sides, the lace holes formed by an oval punch, reducing the cross-section of belt the least, and the lacing put in evenly, of equal strength at the edges of the belt, and no crossing of laces on the inside. If copper, or other rivets, are used, the heads should be "let in" rather below the level of the inside surface of the belt to prevent contact with the pulley, and the washers placed on the outside surface. If the beveled and lapped ends are sewed, the waxed ends should be "laid in" flush on the inside of the belt to prevent wear.

Belts and pulleys should be kept clean and free from accumula-

tions of dust and grease, and particularly from contact of lubricating oils, some of which permanently injure the leather.

Quick motion belts should be made as straight and as uniform in section and density as possible, and endless if practicable, that is, with permanent joints.

Horizontal, inclined and long belts give a much better effect than vertical and short ones, and those which have the driving side below than otherwise.

Belts which run loose of course will last much longer than those which must be drawn tightly to drive; tightness being evidence of overwork and disproportion.

Tighteners should never be used, but when they must be, they should always be as large in diameter, and as free running as can be, and should be applied to the slack side of belts.

The most effective tightener is the weight of the belt on its slack side, which increases adhesion by increasing circumferential contact with the pulleys.

"Belts which run perpendicularly should be kept tightly strained, and should be of well-stretched leather, as their weight tends to decrease their close contact with the lower pulley."—*Hoyt Bros.*

"Belts of *coarse, loose* leather will do better service in dry, warm places; for wet or moist situations the *finest* and *firmest* leather should be used."—*Hoyt Bros.*

"Care should be taken that belts are kept soft and pliable. The question is often asked, 'what is best for this purpose.' We advise, when the belt is pliable, and only dry and husky, the application of blood-warm tallow; this applied and dried in by heat of fire or sun, will tend to keep the leather in good working condition; the oil of the tallow passes into the fibre of the leather, serving to soften it, and the stearine is left on the outside to fill the pores and leave a smooth surface."

"The addition of resin to the tallow for belts used in wet or damp places, will be of service, and help preserve their strength. Belts which have become hard and dry, should have an application of neat's-foot or liver oil, mixed with a small quantity of resin; this prevents the oil from injuring the belt and helps to preserve it. There should not be so much resin as to leave the belt sticky." *Hoyt Bros.*, N. Y.

Substitutes for Leather Belting.

"A contemporary says that the improved steel wire has a strength of from 160 to 175 tons per square inch of actual section; that 176 No. 14 wires have a total section of one square inch, and that each wire will bear from 2,000 to 1 ton leaking strain; and that the ropes made from these wires run readily around 4 feet or 5 feet drums, coil perfectly, and last for a long time. Such being the case it becomes important to us to look to steel, in a measure, to substitute leather driving belts." . . . . . .

"While the substitution of leather by other substances, such as the vegetable gums, gutta-percha and india-rubber, impregnated into strips of coarse woven fabrics, has often been tried, and used, too, with a certain measure of success. Speaking as we now do, from an experiment as to the value of such a combination for driving belts, we can certainly assert that we never found them one-quarter as durable as leather; their use was more costly than the older used substance."

. . . . "In some, though few cases, iron and steel wire belts have been used, the pulleys on which they run being covered with buckskin or some other leather, to increase the adhesion."

"We have cited these few remarks to throw out the hint that, now when cheap and strong steel wire can be purchased, there promises to be a fruitful field for inventive talent to devise some means of so weaving steel wire and gutta-percha into flat belting, producing a stronger and better adhering driving band than leather ever can be."—*Prac. Mech. Jour.*, Nov., 1867, p. 237.

Chas. Sanderson, of Sheffield, England, has taken out a patent—dated Dec. 8, 1862—"For making driving bands of thin sheet metal, coated with rubber to prevent oxidation." "The bands are first well cleaned with acids, then coated by electro process with brass, after which they are coated all over with gum vulcanized thereon, and which adheres tenaciously to the metal coating. Bands of great strength may be made by cementing together several made as above, with a layer of gum between each, the gum imparting flexibility and adhesion to the compound band in passing over the pulleys."

George and Daniel Spill, of Middlesex, England—under date of Nov. 9, 1859—have taken out a patent for "the manufacture of bands by weaving together covered strips of metal with ends of hemp or other fibrous material."

"A strip, or band, or wire of steel is covered with one or more strands of hemp cord, previously passed through a solution of caoutchouc, gutta-percha, glue, drying oils, gums, resins, tar, pitch or other glutinous, gelatinous or siccative materials. After the strands have been applied, the strip or wire is passed between rollers, in order to solidify the covering.

Any required number of metal strips or wires thus covered are used as warps in a loom, and hemp cord or other fibrous material, previously covered with a solution of caoutchouc or any of the other before-mentioned materials, or not, is employed to weave the whole together.

The fabric thus produced is passed between rollers, to render it flat and smooth, and before or after so doing, a solution of caoutchouc, gutta-percha or a coat of paint, or any other desired material is applied thereto.

M. J. Haines, of Stroud, England, has taken out a patent—bearing date Feb. 14, 1860—for making driving belts.

"This invention consists in cutting leather or hides into narrow strips of equal width, each strip width representing the thickness of the intended driving belt, and placing the same side by side, breaking joints with the lengths to make the whole of uniform strength, and with the cut edges of the leather coming to the upper and under surfaces of the intended belt, until the desired width is obtained. The whole are fastened together by wire, rivets or screws passing transversely through the strips, and secured on the opposite sides."

"An interesting description of American belting is made chiefly of wool, and the surface of the belt covered with a resinous cement. We saw a small piece that had been in use for 2½ years on a heavy cloth loom in the States."—*Lond. Mech. Mag.*, Mar., 1863.

For description of a peculiar form of driving belt, the invention of W. Clissold, see *Frank. Inst. Jour.*, Aug., 1863, p. 121, or magazine above. It consists of double links of leather or other similar material connected by intermediate links of metal, the whole series running in grooved pulleys, the leather only touching the sides of the grooves, and driving by adhesion in the same manner as ordinary round belts.

(To be continued.)

THE CORNWALL BRIDGE.

The Bill passed in the State Assembly for the construction of a suspension bridge across the Hudson river, 42 miles above New York city, appears likely to be acted upon, and the designs and calculations for the structure are now almost completed. The total length of the bridge, including approaches, will be 2,499 feet, the length between towers, 1,665 feet; the clear span, 1,600 feet; the height of the towers, 280 feet; distance from platform to water level, 150 feet. One of the towers will be in 30 feet of water, the other will be a land pier. The bridge will be carried by twenty cables, disposed in four systems; each cable will be 14 inches in diameter, formed of steel strands, disposed as in Mr. Roebling's bridge at Cincinnati; these, combined, will require 70,302 miles of steel wire for their manufacture. There will be 58,084 cubic yards of masonry in the towers.

There will be a road platform, as well as a railroad track, which latter is calculated to a working load of 2,400 tons. The platforms of the bridge would be filled by 32 passenger cars, or 53 locomotives, and 18,000 people, whilst the working strength allows for the crowding of 34,560 people and 60 locomotives upon the platforms at one time.

The bridge, which is estimated to cost about 500,000*l*., will connect the mining districts of Pennsylvania with the New England States, and effect a saving of four shillings a ton on the four millions of tons of coal now consumed annually in New England. At present, about a million of tons are carried every year down the Hudson to the depôts along the coast. By means of a short branch made to the bridge, the Erie Railway will be enabled to obtain a station in New York, and be saved the expense and inconvenience of transferring goods and passengers by ferry to their terminal station in Jersey City.

Pending the completion of the bridge, a ferry will be established at the point of crossing, for the transfer of the traffic, as the railway will be completed up to the east and west banks of the Hudson long before the permanent connection can be made.—*Engineering.*

EXTRACTS FROM AN ENGINEER'S NOTE-BOOK.

By W. M. Henderson, Hydraulic Engineer.

By particular request, the following notes, relating to the subject of steam boilers, are offered to those interested in the matter of the late steam boiler inspection law. The information conveyed has been condensed mainly from the works of William Fairbairn, Robert Armstrong, and Charles Wye Williams, interspersed with the results of twenty years practical experience of the writer in the premises.

Chemistry of Combustion.

Ordinary combustion is the combination of oxygen with the combustible element of fuel. In coke and charcoal, carbon is the sole combustible element; while in coal there is hydrogen also. In 100 pounds of good coal, there are about 84 pounds of carbon, and 6 pounds of hydrogen; the residue is composed of matter that does not assist combustion, but sometimes retards it by forming clinkers, and otherwise obstructing the process. The hydrogen furnishes weight for weight, about four times as much heat as the carbon, or 22 per cent. of the whole. The products of combustion are:

Steam............	H 2, 0 1	in weight as 1 to 8,	invisible and incombustible.
Carbonic acid..	C 1, 0 2	" " 6 to 16,	" "
Carbonic oxide,	C 1, 0 1		invisible and combustible.
Smoke			

The first part of the process of burning coal, consists in distilling, or expelling by heat, the hydrogen, in the gaseous form, combined with the carbon in one or two proportions, forming carburetted hydrogen, or coal gas, H 2 C 1 in weight, as 1 to 3, and bi-carburetted hydrogen or olefiant gas, H 2 C 2 in weight, as 1 to 6, the latter forming 10 per cent. of the whole. Taking this estimate, we find that nine-tenths of the six pounds of hydrogen combines with three times its weight of carbon, taking up 16·2 pounds of the latter, and one-tenth combines with six times its weight, taking 3·6 pounds, together taking 19·8 pounds from the 84 pounds of carbon in the 100 pounds of coal, making 25·8 pounds of carburetted gases, to be burned above the coke, leaving 64·2 pounds of carbon to be burned in the form of coke.

Now to burn one pound of hydrogen requires eight pounds of

oxygen, and the product is nine pounds of aqueous vapor; and to burn one pound of carbon, requires two and two-third pounds of oxygen; the product being three and two-third pounds of carbonic acid (the resultant of perfect combustion). Hence to burn the six pounds of hydrogen requires 48 pounds of oxygen; and to burn the 19·8 pounds of carbon, combined with it, requires 52·8 pounds; altogether 100·8 pounds of oxygen to burn the gaseous portion of 100 pounds of coal. To burn the 64·2 pounds of carbon that remains upon the grate, after the expulsion of the volatile gases, would require 172·2 pounds of oxygen, the sum of both being 272 pounds of oxygen to consume 100 pounds of coal.

Atmospheric air consists of volumes N 4 0 1 in weight as 28 to 8; the oxygen being but one-fifth of its bulk, and the quantity required being as 2 to 1 of the carbon, in order to produce perfect combustion; the quantity of air employed will therefore be ten times the volume of the gas to be consumed. From this it will be seen that 1,224 pounds or 16,320 cubic feet of air must be admitted to effect the combustion of 100 pounds of coal, allowing that 60 cubic feet of atmospheric air are required to produce each pound of oxygen. The quantity chemically required for one pound of coal is therefore about 164 cubic feet, of which 60·8 enters into combustion with the volatile gases, and 103·2 with the solid portion of the coal. At the general temperature of the furnace, 1,000°, these products of combustion expand about three times the original bulk of the air, *i. e.* $164 \times 3 = 492$ cubic feet of air and gas to be passed with a velocity of 36 feet per second.

The formation of carbonic oxide is known to be greater in a thick fire with a poor draft, than in a thin fire with a strong draft; in the former case, there may be perfect combustion, producing carbonic acid in the lower part, and the acid so produced may take up another equivalent of carbon in the upper part of the fire; and thus waste half of the carbon, which passes off in the form of carbonic oxide. The remedy for this is a judicious introduction of air above the fire. But to effect the combustion of either gas or solid carbon, it is necessary that the oxygen which is to combine with it, should be brought within the sphere of its attraction; the two must be brought into intimate contact and touch, or nearly touch, while the temperature is favorable. The gas from a burner or candle will not attract oxygen at one-eighth of an inch distance; practically, they must be intimately mixed, before they can burn.

If these conditions be not fulfilled, the combustion will be incomplete; the hydrogen, possessing the strongest affinity for oxygen, will first combine and leave the carbon free, in the form of black powder, which will mix with the vapor resulting from the combustion of the hydrogen, and with the nitrogen gas, and constitute what is called smoke. Now, if this smoke, containing, as it often does, valuable quantities of uncombined carbon, comes in contact with cold surfaces, it loses heat, falls to the temperature of steam, and its combustion is then practically impossible.

At the moment the hydrogen and oxygen combine to form water, they are at a white heat; if, at this moment, there be present a sufficient excess of oxygen, the carbon will be consumed; but if at this moment, the carbon is not also combined, smoke ensues. The problem is to burn the gas in the act of distillation at one operation.

Philadelphia, February 6, 1869.

(To be Continued.)

BRIDGE AT OMAHA, (U. S.)

One of the most important works on the Union Pacific Railroad—the construction of a bridge across the Missouri river, at Omaha, 400 miles west of Chicago—is about to be commenced by General G. M. Dodge, engineer of the Union Pacific Railway. The bridge is about 2,800 feet long, and is divided into eleven spans of 250 feet each, the piers being cylinders of cast iron, 8 feet 6 inches in diameter, and filled with concrete. The treacherous bottom of the Missouri river presents more than ordinary difficulties in obtaining a reliable foundation, from the great depth of the shifting sand, which is constantly filling up old channels and opening fresh ones, so that the section of the bed is ever varying. Where it is possible, the cylinders will be lowered on to the rock, and elsewhere to a depth of 70 feet below low water, in the sand, the bases being enlarged from 8 feet 6 inches to 12 feet in diameter, to spread the bearing surface, which will also be increased by flat bars projecting from the foot of the cylinder into the surrounding sand. Foundations of this class have been successfully employed by the Hon. W. J. McAlpine, in various bridges he has constructed. The length of the cylinders from low water to the underside of the girders will be 69 feet, making a total height of the main columns of 139 feet.

The ten piers, each with two cylinders, will be braced traversely, and protected up stream with ice breakers attached to columns 5 feet diameter, and placed 20 feet in advance of the piers. The faces will be of cast iron plates, meeting at an angle of 45 degrees, in front of the columns, to which they are braced with oak timber, the intermediate spaces being filled with rubble and concrete. From below low water to the highest flood levels, the cylinders will be cased by plates, and the enclosed space will be filled in with concrete, to prevent any accumulation of ice, or other obstructions which may be carried down the stream, from getting between the cylinders, and straining them on the intermediate bracing.

The girders of the superstructure will be trusses made of wrought iron, with the exception of a cast upper chord.

The approaches to the bridge on both shores will be on a gradient of 1 in 30, made in embankment on the eastern side to a height of 40 feet above the ground, the remainder being a viaduct of trestle work. The total length of the whole, including the river crossing, will be about 3½ miles.

Four railway companies are subscribing the funds for this bridge —the Union Pacific, the Chicago and North-Western, the Chicago and Burlington, and the Chicago and Rock Island Railroads.—*E.*

AMERICAN RAILS.

Mr. C. P. Sandberg, whose paper "On the Manufacture and Wear of Rails" just published in this *Journal*, excited such a prolonged discussion at the Institution of Civil Engineers last session, has addressed a letter to the *Times* condemning the system of ordering rails followed by the American railways. Our space will not permit us to publish Mr. Sandberg's letter *in extenso*, but we subjoin an abridgement which will be read with interest. Mr. Sandberg's remarks are very just and well deserving of attention. He says:

"The term 'American rails' has become a synonym for the cheapest and least durable rails manufactured. They are usually about ten shillings per ton cheaper than the ordinary rails made for English and Continental companies. In the case of American rails, the quality of the material and the construction of the rail pile are

left entirely to the manufacturer, the rails not being made according to any specification; and hence there is not the slightest guarantee that a good, serviceable, or safe rail will be obtained; the one great desideratum being, apparently, that the price be low. Hence, the maker's chief study is naturally enough to produce the cheapest possible article, and to devise means of manufacturing at a low price what is, to all appearances, a clean looking rail. To do this, he carefully studies the character of his iron, and so manipulates it as to obtain a well finished and saleable rail, regardless of its brittleness—so long, indeed, as it does not break previously to delivery and payment—and indifferent whether it is likely to last one year or ten. Fortunately for him, the section for American rails is one very easy to roll—low, heavy, and without angles—so that almost any quality of iron and any construction of pile will not interfere with the one object he has in view. When, however, the iron is very red-short, (or liable, through the presence of sulphur, to crack in rolling) a top-slab of a better class of iron (No. 2) must be used in the pile to serve as the wearing surface of the rail. This wearing surface may, however, vary considerably in thickness, forming either the entire head of the rail, or only a portion more or less thick. Even when the iron is not red-short, the pile is often composed of puddled bars only, and rolled out into rails at the lowest possible heat, so as to economize iron and fuel, but regardless of insuring a perfect weld; and hence, lamination and failure rapidly follow after a few months' wear.

"So much for the durability of the ordinary American rail. Now as regards its safety. Just as the presence of sulphur in iron renders the metal red-short, as previously explained, so the presence of phosphorus causes the iron to become brittle and cold-short. It is not, therefore, of great importance in producing a good and serviceable rail from such inferior materials, that the hard, cold-short iron should form the top, or wearing portion of the rail, while the red-short, or tough and fibrous iron, should be used for the flange. As the character of the ores distributed through the principal rail-making districts of this country is such that cold-short iron is produced in one district and red-short in another, it is necessary that the two kinds of metal should be brought together and used in association, as previously described, if they are to produce a truly serviceable rail. But as the cost of transport from one district to another becomes an important item, it will evidently be to the in-

terest of the manufacturer, if not restricted, to use the unmixed home material, whether cold-short or red-short. Under such circumstances a rail is produced either too brittle, and therefore dangerous, or too pliable, and therefore less capable of enduring the wear and tear of traffic. There are, perhaps, few countries that of late have suffered more from fracture of rails than America. This has led some railway administrations in that country to require that the rails should be tested ; but whereas they were formerly too careless in this respect, they now seem inclined to err on the other side by specifying too severe a test for the rail, and thus compelling the maker to use too soft an iron. For instance, it is often required that a weight of one ton should fall upon the rail from a height of 10 feet, when half such a test would insure breakage of the rail in any climate. I may now briefly refer to the method adopted in making rails for the English and Continental companies. There are but few of these railway administrations which, when inviting tenders for a supply of rail, do not specify distinctly that the top slab, constituting the wearing surface of the rail, must be of the very best material, and at least 2 inches in thickness, thus giving a wearing surface of $\frac{1}{2}$ inch in the head of the rail; and, further, that the rail should stand a test half as severe as that previously mentioned as applied to American rails. From what has now been advanced respecting the different modes of manufacturing American and European rails, I leave the respective American railway administrations to judge whether they would not best consult their own interests by adopting the English and Continental system of well-defined specification and tests, instead of looking merely to the small saving effected by always accepting the lowest tender."—*E.*

SUBMARINE BLASTING.

The East river entrance of the New York harbor is obstructed by the presence of sunken rocks, which render the navigation of deep draft vessels a matter of considerable difficulty, and of no little danger. Sixteen years ago the obstructions were partially reduced by blasting, but the process, which consisted of lowering charges of gunpowder upon the rocks, and igniting them by a battery through connecting wires, served only to remove such salient points as presented themselves to the action of the powder.

The improvement of this portion of the harbor has lately again occupied the attention of the United States Government, which has appropriated 17,000*l.* to this object.

Preparations are now being made for the removal of one of the principal obstacles known as Hell Gate, and special apparatus has been designed for drilling the sunken rocks for the introduction of the charge. The principal part of the machine is a water-tight iron casing, in form a depressed semi-spheriod, 7 feet in diameter. It has three solid steel feet or toes by which its stability on the rock is secured. Rising from the upper part of the casing is a conical wrought-iron frame, supporting the upper end of the drill shaft by means of two parallel rods entering into sockets in a cast ring at the top of the frame. The drill bar passing up through the centre of the top is furnished at the bottom with a bit, one and half inches diameter, having imbedded in its face nineteen diamonds, and rotating at the rate of from 300 to 500 revolutions per minute, advancing at the rate of from one to one and half inches in the same time.

The feed is caused by a differential gearing which steadily operates to advance the drill into the rock, the débris being washed away by the water forced into contact with the bit through a small rubber hose. The water-tight chamber of the machine contains a pair of engines working at right angles to each other, with an horizontal stroke. As soon as the hole is completely drilled, and also when the drill shaft is withdrawn from the rock, information of this is given by a magnetic bell which is acted upon by a double wire cord insulated from the water and passing down one of the parallel rods or tubes upon which the crosshead is fixed.

This drill weighs nearly five tons. It will be worked from a wrecking tug with a derrick, by means of steam supplied from the boiler of the tug. To prevent this steam being condensed in its passage through the water to the engine, it is conveyed in a hose surrounded by another, through which the exhausted steam passes.

The rock which is to be drilled in the Hell Gate is that known as the "bastard" granite, and is much softer than either the Quincy or Maine granite, on which the drill has been satisfactorily tested. After a number of holes are drilled over a certain space, a diver will descend and charge them with cartridges of nitro-glycerine, which will be exploded in the usual manner. The fragments will be raised by automatic grapnels.—*Engineering.*

PORTLAND CEMENT.

WITH the enormous stores of the raw material for manufacturing cement, possessed by England, together with the advantageous position she occupies with respect to fuel, it is no wonder, accustomed as we are to be foremost in all matters of a remunerative commercial character, that we have almost a monopoly of this indispensable aid to construction. Mr. Reid has followed in the wake of Mr. Grant, and has added to our very scanty practical information upon the manufacture of Portland cement by the excellent treatise* he has lately given to the public. Without desiring to accuse our engineers and architects of being remiss in their duties, yet it must be acknowledged that the credit is due to our professional brethren on the other side of the water, for directing attention to the admirable qualities possessed by cement for foundations under water and damp and spongy situations. As a rule, the French were long before us in the use of cement, concrete, "pierre perdue," bêton, aggloméré and, in fact, every description of artificial masonry. We for some time refused to abandon stone, and bricks and mortar, and adhered to the ancient types of construction, with true Anglo-Saxon obstinacy and perversity. Gradually, however, we were brought to see the error of our ways, and the commercial element mingling strongly with the adoption of cement, we now not only manufacture it on a very extensive scale, but use it also to a considerable extent, in subaqueous and other works. The application of this important material appears to be almost illimitable even at present, and it promises to embrace a still wider sphere of action. All the engineers, foreign as well as English, concur in commending it highly in whatever works they have availed themselves of its services; and now that it can be obtained of the strength and quality required, there is no longer any doubt remaining regarding its being a thoroughly reliable and trustworthy material.

Mr. Reid enters at full length into the practical manufacture of the article, and describes in detail the various processes it has to pass through before its raw constituents, the chalk and the clay,

* "A Practical Treatise on the Manufacture of Portland Cement." By HENRY REID, C.E. To which is added a Translation of M. A. LIPOWITZ's Work, describing a New Method Adopted in Germany, of Manufacturing that Cement. By W. F. REID. London: E. and F. N. Spon, 48, Charing-cross, 1868.

can be presented to the public in a marketable form. Washing, mixing, burning and grinding constitute the principal phases of the ordeal it is submitted to, and in the first and second of these, which are really accomplished at the one and the same operation, there exists a difference between the plans adopted by the English and the German manufacturers. The former use the wet and the latter the dry systems. Each of them have certain advantages of their own, and it is perhaps as much a matter of habit and national individuality as of any practical superiority. Our author, on the whole, gives the preference to the wet system, although he fairly admits all the advantages that may be possessed, and are urged in favor of the German method. The necessity for sampling the cement, that is, testing it after the operations of washing and mixing have been carried out, is strongly insisted on as one that should never be omitted, for if the proper proportions in which the chalk, clay and water should be mixed, are not discovered at this juncture, it will be too late to remedy the evil afterwards. Two descriptions of tests, namely, the water and the air or dry test, are employed, and the experienced hand can tell by the united aid of the two, whether the mixture has been properly accomplished, and whether it should be allowed to pass on to the kiln. With respect to this latter part of the machinery employed, it does not appear to have arrived at the condition of comparative perfection it should have done. The original lime-kiln shape has not been much departed from, and the whole operation is conducted in a very rough, unscientific manner. In the appendix—or, rather, the second portion—of the volume, a description is given of an endless kiln, which would supply some of the deficiencies found in those of ordinary construction. Notwithstanding the practice and experience that the author has had in the matter, he confesses that it is quite impossible to calculate the time for burning a kiln, and he mentions an instance that came under his own immediate knowledge, where the difference between the periods of burning the same kiln was as four to one. After noticing the grinding process, and the machinery adopted for effecting that somewhat arduous task, the author proceeds to the question of testing the cement. It is not too much to assert that had it not been for the Metropolitan Board of Works, and the energy and decision with which Mr. Grant carried out the provisions of the main drainage contracts, we should never have been able to procure the cement that can now be had with facility. It was at one

time considered impossible to manufacture it of the strength required, and some firms openly expressed their disbelief in the possibility of the results. But after some quiet discussion on the matter, and finding that the engineers would not yield, they betook themselves seriously to the task, and speedily discovered that they had been making a mountain of a mole hill, and that the difficulty was easily surmounted. The needle test is still used in France, but it is fast becoming altogether obsolete, and has been replaced by the tests referring to the tensile strength of the cement, the relative solidity or resistance to fracture, and the compressive strength or resistance to a force tending to crush it. In Germany, they test Portland cement against equivalent blocks of stone and brick; but this is evidently but comparatively worth little, since no two specimens of either stone or brick possess constant qualities in this respect. With regard to tests for durability, the cement has so lately been manufactured of a really reliable character, that it is premature to indulge in them.

Most of the applications of cement, whether in its pure state, or any one of its numerous combinations, are known to our readers, especially its recent application to the construction of dwellings, warehouses, and other buildings of large size. There has one been made which is not so very generally known. It is the application of the material to prevent oxidization in the holds of iron vessels, and also to the external use of it in coating ships. Captain Cowper Coles, C.B., has applied it in the latter manner upon several vessels, and apparently with much success. Another comparatively novel method of utilizing this valuable material, is in concrete for the construction and repair of the surfaces of roads. Although, partially, this experiment has been successful, it has also failed lamentably in one or two instances. Mr. Reid candidly admits that it proved a failure in St. James' Park, although he gives a ready explanation of the cause of failure, of the reason why the surface broke up under the action of the rapid traffic over that part of the Mall. We do not consider that a second attempt would also necessarily prove a failure; on the contrary, it would probably turn out a success, but until further experiments have been undertaken, and more reliable data supplied, we should be careful of expressing a decided opinion.

The second portion of this valuable treatise is devoted to a translation of the work of M. A. Lipowitz. It is in a more condensed

form than the preceding portion, and it is not by any means of the same value. In fact, it would do the work not the least harm if it were omitted altogether, as a portion of it must, of necessity, be repetition. It treats, principally, of the German methods and class of machinery, and contains some good plans and sections of those employed in that country. Mr. Reid's volume is, in fact, the only book of its kind, and one very much wanted. It is true that the experiments of Mr. Grant cannot be overrated; but they do not supply the practical information afforded in the volume in question, which is a complete guide for the manufacturer, authority for the professional man, and a book of reference to every one who desires to become acquainted with the theoretical and practical history of Portland cement. The plates are very well lithographed, and the type clear and well chosen. It is not often that we have reviewed a book with so much pleasure as at present, and we can cordially recommend it as a capital text-book and manual of the subject upon which it so ably treats.—*Lond. Mech. Mag.*

Railway Management in Egypt.—Mr. McGregor, the well-known owner of the Rob Roy canoe, has been recently making explorations in Egypt and Syria; and in the course of an interesting letter to the *Times* describing his adventures, he speaks as follows as to the management of the Suez and Cairo Railway:—"My Canoe was carried by railway from Suez to Cairo with much difficulty and expense. The natives laughed at me for 'buying tickets' as a passenger, it being the constant practice on that line to bribe the guard with five francs and travel as one likes." We should think that a guard's situation on the above line would be a fine opening for a man not troubled with too sensitive a conscience.

Mechanics, Physics, and Chemistry.

THE BEST MODES OF TESTING THE POWER AND ECONOMY OF THE STEAM ENGINE.

By Charles E. Emery.

Late of the U. S. Navy and U. S. Steam Expansion Experiments.

(Concluded from page 120.)

Having discussed the various measures and means that may be employed for our purpose, we desire next to select such as will be useful in particular cases, and show their practical application—which leads us to

The methods of conducting Experiments.—I. Testing Boilers.

The power of an engine can never exceed that of the boiler which furnishes it with steam; hence, it is eminently proper that we should first select measures to ascertain, in a given instance, whether the steam is economically generated. As has been said, the heat-producing power or evaporative efficiency of a boiler is measured by the number of pounds of water evaporated per pound of coal from a given temperature, say 212 Fahrenheit. We have, therefore, to weigh the water evaporated and the coal producing the evaporation—a very simple thing, apparently, but one about which there is much misapprehension, resulting in statements grossly erroneous and ridiculous. The water may be measured in a tank or barrel, the contents of which has been ascertained, by careful measurement, or by weighing water into it of a given temperature. When experimenting, the water in the tank should be pumped out dry, if possible, or at least to a given mark—the pump then stopped, the tank refilled to the proper height (the easiest way is to overflow it), when the supply can be shut off and the operation repeated. The supply pipe should be arranged so that the water can be seen entering the tank, and leakage detected while the pump is working. The better way is to have a hose, to throw in and out of the measuring tank. Before making an experiment, it should be

ascertained if the boiler foams, or raises water; if so, it must be remedied before proceeding farther. All leaks about the tank, pump, and boiler should be stopped; and all extra pipes leading water in or out of the boiler be disconnected, or frequently examined. The steam generated may be worked off in the engine, blown off through the safety valves, or otherwise disposed of, so long as no water is lifted with it. The latter is less liable to happen when the evaporation takes place under considerable pressure. The greatest care is necessary in commencing and ending experiments. There are several methods of doing this. The first is, to measure the temperature and height of the water in the boiler, and immediately upon starting the fire, to keep an account of the fuel consumed until the close of the experiment, then to weigh the coal and ashes hauled out of the furnace. This involves a calculation to ascertain the heating effect of the fuel used in generating steam. It is of little value for the purpose of comparison, for the shell of the boiler and its surroundings (often a heavy mass of brick work) has also to be heated, and of this no estimate can be formed. Another plan often adopted is to get up steam with wood, and allow it to burn low, leaving only sufficient fire to start the coal. The experiment is started when the first coal is put in the furnace, and terminated when the last coal is nearly burned. This plan is supposed to give an accurate measure of the coal burned. The better plan is, to get everything in average working condition before starting to experiment. The steam should have the proper pressure; the fire be clean, and of a certain thickness, judging by marks on the sides of the furnace; the ash-pits clean, and the water at a certain known height. The experiment may then proceed, weighing all the coal afterwards used, and measuring the water pumped into the boiler, till near the desired time to stop, when the fire should be thoroughly cleaned, and filled up with coal to the same marks as at the beginning, and should be maintained at that point, with the steam at the starting pressure, till after pumping in the last tank of water, when, as soon as the water level reaches the same height as at starting, the experiment may be terminated. The ashes in the pit should then be weighed, as well as those previously collected. The fire should be equally bright, and the steam pressure the same at the beginning and end of the experiment, so that the water level will be disturbed in like manner. At starting and stopping, a certain feed should be kept on, or the water should be

pumped too high, and time noted when, by evaporation, the level falls to the mark. No experiment should be less than eight hours in length, and a trial of forty-eight to seventy-two hours duration can better be depended upon. During the experiment a log should be kept, upon which should be recorded the time, the weight of the coal and ashes, the number of tanks of feed water, and the temperature of each. The temperature of the escaping products of combustion and of the fire room may also be noted, as well as any evident remarks about the kind of coal, and the circumstances of the trial. After the experiment, the following calculations are necessary: First, in an evident manner, ascertain the total amount of coal and ashes; subtract one from the other, which gives the total weight of the combustible. Then find the average temperature of the feed water and the average pressure of steam, and calculate the weight of the whole quantity of water evaporated, making allowance for its temperature.

The next step is to find the quantity of water evaporated from a constant temperature, say 212°. From formula or tables find the total heat of the steam due to its mean total pressure; from this deduct the total heat which the water contained before entering the boiler. The result is the number of units of heat imparted to each pound of water. Divide this by the latent heat of steam at 212°, and multiply the quotient by the total number of pounds of water evaporated at the observed pressure; the result will be the total evaporation from our supposed temperature of 212° and at atmospheric pressure. The latter divided by the total amount of coal burned, or, if desired, by the combustible, gives the final result, in the usual comparative terms, viz: *the number of pounds of water evaporated per pound of coal* (or combustible). The coal may be corrected to a uniform rate of 10 per cent. refuse, as has been before explained.

We have reason to suppose that in many experiments abroad, the ashes were "weighed back," and credited on the coal account: in other words, that what is reported as coal was really only the combustible portion thereof. In purchasing coal, we pay as much for the ashes as for the combustible, and ships must carry both, in a combined state: therefore, the report of every experiment should clearly state what is meant by the word COAL, if that be the term employed, whether the weight of the coal as actually purchased, that of its combustible, or a weight proportioned to the combustible, on our plan of correcting to a standard of 10 per cent. refuse.

II.—Testing Engines.

We will examine, first, a simple mode, which may be practically applied, in any case, to test the economy of steam machinery, in the actual performance of its regular duty. In ordinary trials, when but little care and expense can be afforded, the engine and boilers must be tested as a whole, the comparison being made by the "*number of pounds of coal consumed per indicated horse power per hour.*" The indicator is used to measure the power, because, as has been before explained, it is the simplest device we have for this purpose, and most generally applicable. The results will be of little value, however, except under the conditions hereinbefore expressed. We first desire to give, from our experience, some directions about the use of the indicator and the manner of attaching it to the engine. Since the invention of the "Richards" or "Porter Indicator," the direct-acting instrument known as the "McNaught Indicator," has fallen into disuse, except on engines working very slowly. We will make our remarks more especially applicable, then, to the first instrument, often called the "parallel motion indicator." Before using the instrument, see that it is correctly made and in good order. To do this, examine the piston, see that it moves freely, without shake, through the entire length of the cylinder. See that the spring screws down squarely on the piston, and does not tend to one side, and thus make friction in the guide of the piston rod. Examine every joint, and see that it is free, without shake; see if the two links are parallel at all times, and the radius arms at mid position—if not, the arrangement is not a parallel motion, and must be corrected. See that the arm carrying the levers has no vertical shake; see that the barrel runs true, and adjust a pencil in place to bear lightly upon it. The scale of the indicator should be tested by a mercury gauge, and the mark on the spring corrected accordingly. This is important, for the reputed scale is rarely correct, and during repairs it is often varied. The instrument should never be connected to the cylinder *ports;* nor in any position where a current passes the connecting pipe. The connection should be large, short, and direct. Be careful to give the barrel the correct reduced motion of the engine piston. Other details may be arranged as convenient. The instrument should be thoroughly heated before taking a diagram or marking the atmospheric line. The pencil should be made to bear as lightly as it

will make a mark, and it should be allowed to run over the paper several times. Both ends of the cylinder should be indicated.

Before beginning an experiment, both engine and boiler should be in average working condition. At the commencement the fire should be clean, and its thickness noted. The contents of the ash-pit should then be removed, and the coal be weighed, the same as in testing boilers. Indicator diagrams should be taken once an hour, or every half hour, or even less, if the load varies considerably. The pencil should be allowed to remain on each diagram a considerable time, to get a fair average. A register or counter should be attached to the engine, the indications of which should be noted at the beginning and end of the experiment, and every even hour intervening. If a register cannot be obtained, the revolutions should be counted and recorded every fifteen minutes. This should be continued not less than eight hours, and a longer time is preferable. At the end of the experiment the fire should be clean, and of the same thickness as at the beginning, the same as in testing boilers. A log should be kept during the progress of the experiment, showing the time, pressure of steam, revolutions of engine, weight of coal and ashes, and other matters of interest. The calculations are simple, and need not be detailed. We will here remark that the fault with most experiments is the short time for which they are tested. To ascertain accurately the consumption of fuel in a given case, requires, as has been said, at least eight hours continuous action, and the mean power cannot be obtained, in many instances, in much less time. A single diagram, taken occasionally, gives little idea of the actual power exerted, for in every manufactory the load is constantly changing. It is more than probable that the excellent results claimed in many cases are obtained by calculating the power from a diagram taken with the full load on, and the cost of the power from the average coal, or worse yet, from the coal which is thrown in the furnace in any particular hour, without noting whether the fire is heavier at the beginning of the hour than at the end. A manufacturer's coal bills always tell him what his steam power has cost for a given time, but his one hundred horse power engine might have been exerting, on the average, only fifty horse power; so, without actual and careful observation, no results can be obtained of any value to the engineering profession. The only true way is to make thorough trials, and repeat them until the results practically coincide.

When the power of the engine is measured by a dynamometer, the same care should be taken to frequently record the revolutions of the engine and the indications of the instrument, so as to be able to calculate the true average power. Fuller reasons for such precautions have already been given in the preceding discussion.

We are now prepared to select the methods and means necessary for a scientific trial of the economy of steam machinery, which shall be complete, and above criticism. We must first bear in mind that it is the *economy* that we wish to test, and not the excellent manner in which some device controls the speed of the engine, under varying loads. Special trials may be made of each detail, if desired, but only one thing can be tested at a time. To get accurate results, great uniformity is necessary. The closer the resemblance between the records, at different times, the more correct will be the averages. It is essential, then, to carry a uniform pressure of steam, and to have a uniform load and speed to the engine. In regular practice the load is necessarily varied somewhat, which can only be provided against by frequent observations; but our remarks are more particularly applicable to an establishment fitted up especially to test steam machinery, and in other trials details must be varied according to circumstances. In such case the boiler should be of ample size to do the work, and the pressure should be regulated by a steam damper. The resistance should consist of wind or water wheels, or pumps. We prefer high speed fans or blowers, as the resistance can then be easily regulated by varying the size of the discharge openings. Tanks should be provided for measuring the feed water of the boiler, and it would be well, though not strictly necessary, to have a surface condenser from which to collect and measure the distilled water, and thus, in two ways, ascertain the quantity of steam used. The power of the engine should be measured both by the indicator and dynamometer, and duplicate registers should be provided to count the revolutions. The better plan, in order to give the same area of indicator diagram, is to use, in each experiment, a cut-off, fixed at any desired point, and *not* use the governor. In such case, special means must be provided to keep up a uniform lubrication, which, with the uniform resistance proposed, will secure uniform speed.

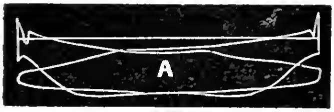
When experimenting, the coal should be weighed, and the feed

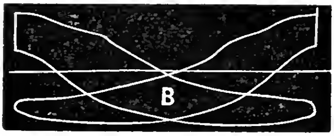
water measured, or weighed, with all the accuracy required for testing boilers. At the same time, indicator diagrams should be taken at least once an hour, and the reading of the dynamometer recorded. A record should also be kept of the time, revolutions of engine, steam pressure, and the temperature of the feed water, and in a condensing engine, of the hot well and circulating or condensing water. The temperature of the engine and fire room, and of the external air, should also be noted, to show the effect on condensation in the pipes and passages. The direction and force of the wind is also useful, to show its influence on the fires. Barometrical observations are essential, to show the true zero of the steam pressures. Experiments conducted thus carefully, and with such apparatus, would furnish results of the greatest value to science. Each trial would show the economy of the boiler and of the engine, also the friction of the engine and its load, and the net power and its cost; besides affording much valuable information to aid in the explanation of the losses which now exist in the steam engine, and suggesting improvements in its construction. The United States Expansion Experiments were tried, substantially, on this plan, but were stopped when results were being obtained of the greatest interest. Could an experimental establishment be now opened to manufacturers and inventors, how much capital, physical exertion, and mental anxiety could be saved, and how greatly the steam-engine might be improved. Without such a place, however, much good can be done, if every engineer will carefully use the means at his command, and record the results. The awards at all our Fairs should be based upon trial, and not upon mere opinion.

The "Yankees" are an ingenious people. Let all assist in directing this ingenuity into scientific channels, and the character of the result may be judged from the present advanced position of our high-pressure engines. By fully discussing the subject of economy, and generally circulating complete records of competitive trials, an important branch of industry will be stimulated, all classes benefitted, and American Engineering become the standard throughout the civilized world.

Note.—The following diagrams were selected from a number taken by the writer to test the accuracy of the steam engine indicator, under the circumstances mentioned on page 51. The pres-

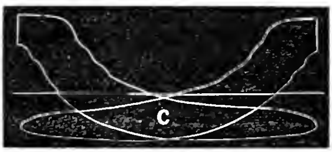
sure of steam in the boiler, and the speed and load of the engine, were the same in all cases. The only conditions changed were first, the point of cut-off, by shifting the link, and second, the amount of throttling which was adjusted to make the speed equal in all cases. An indicator of the "parallel motion" variety in excellent condition, was used in all the experiments. The scale of the instrument was sixteen pounds to the inch, and the speed of the engine during these trials was fifty-one revolutions per minute. Under such conditions, when the actual power exerted was of course the same in all cases, the indicator showed the following:





Mean	pressure	Diagram	A, 8·6	pounds,	Relative	indicated	power, 100
"	"	"	B, 9·45	"	"	"	" 109·9
"	"	"	C, 11·06	"	"	"	" 128 6

These remarkable discrepancies can be attributed to nothing but the indicator itself. The experiments were several times repeated, and every precaution was taken to get accurate results. Confirmatory experiments were tried also under different conditions. The discrepancies above shown are greater than would ordinarily be the case, these diagrams being selected because the differences can be detected by the eye without measurement. The increase shown at the shorter points of cut-off is due as has been said to the inertia and friction of the moving parts of the instrument, but chiefly, we suppose, to the latter, as the variation caused by the first nearly corrects itself in vibrations. Had the mean pressure been greater, the discrepancies from using the same instrument would have been but little more, and would therefore have been of less relative value. Again, the cushioning, shown in the diagram, undoubtedly causes double the amount of discrepancy which would be found in an engine making the same steam line, but having a more uniform exhaust pressure due to independent valves.



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ON THE IMPORTANCE OF AQUEOUS VAPOR IN WARMING AND VENTILATING DWELLING-HOUSES.

By Charles M. Wetherill, Ph. D. M. D.

Professor of Chemistry in the Lehigh University.

The importance of the vapor of water in the atmosphere of our dwellings during the winter season, is not denied by any one; but the proper means for securing this condition is neglected, even by those who acknowledge its importance, and in the various appliances for hydration in the construction of hot air furnaces the means adopted are entirely inadequate.

The writer has had his attention forcibly directed to this subject by a series of experiments and analyses made in Washington, in 1866, upon warming and ventilating the Capitol (House of Reps., 39th Congress, 1st Session, Ex. Doc. No. 100), and now again in his own dwelling, and also in the new university building, Packer Hall, which is warmed by an abnormally dry air, made so by passing over steam coils, as in the capitol.

He has thought that a few remarks would not be out of place in this connection, and he would feel amply repaid if any of our practical men, profiting by the suggestions, would supply the deficiencies complained of, and advance their fortunes by the invention of a hot air furnace constructed upon scientific principles.

This problem has never been solved practically, and yet it admits of a solution.

A great difficulty meets us in the outset, viz: an inertia to be overcome in the public mind, as well as in the opinions of practical men, with respect to the requisites of a more perfect system of warming and ventilation. Thus a class of inventors direct their attention to the construction of the stove to ensure economy of fuel and rapidity of warming the air, and another class discuss the merits of a downward or upward ventilation, while all (even those who admit the necessity of it), neglect the consideration of the best means of effecting the hydration of the air, at least in dwelling-houses, and of the actual amount of water necessary for the purpose.

As a result of the experiments at Washington, after much money had been spent for the investigation, and after it had been proved conclusively by the experiments that the warmth and ventilation

were abundant (even excessive), and that the sole cause of discomfort arose from too low a relative humidity in the air, Congress could or would only appoint a fresh committee to inquire into the means of procuring a more efficient *ventilation* for the Capitol!

In the furnaces which admit the necessity of water, a small panful is afforded, without any further consideration of its efficiency, when the dried up feelings and warped furniture of the inmates of the house are protesting constantly as to its inadequacy.

We use the term "*relative humidity*," to designate the percentage of water which air of a given temperature holds in solution as vapor. Thus, when air of any given temperature contains all of the moisture, which it is able to hold as vapor, we say its relative humidity is 100; if it contains half as much, its relative humidity is 50; relative humidity 10, indicates that the air has 10 per cent. of the vapor of water, which it is able to hold *at that temperature.*

It is a property of air that the quantity of water which it may dissolve to saturation, *increases with the temperature* of the air. Hence, air which is saturated with water at 70° (*i. e.* has a relative humidity of 100), may still have a relative humidity of 100° (*i.e.* be still saturated with water), when cooled down to 32° but does not contain so many grains of water per cubic foot as it did, a portion having been precipitated or condensed as drops of water by the effect of the cooling. Conversely, air having a relative humidity at 32° of 100, has its relative humidity reduced to 50 by being raised to a temperature of about 50° Fah.; or to 25, if it be heated to about 72°.

When the air is moist, we say that it has a high relative humidity; and when its relative humidity is *low*, we say that the air is dry.

What is the effect of these two conditions upon the human body? When the air is saturated with moisture the evaporation from the skin and lungs is arrested, and those interior movements of the bodily fluids necessary to health are restricted. The body is in the condition obtained by the Russian bath, but without its elevated temperature; is in an unnatural condition. The amount of animal heat removed by the evaporation of the corporeal moisture is no longer withdrawn. Even the beneficial effects of the Russian bath are wanting, for the shock to the nervous system is absent.

When the body is in air of too low a relative humidity, the opposite conditions are present. An abnormally large amount of heat is abstracted by the evaporation; the motion of the corporeal fluids

is excessive; the necessary moisture is removed from the lungs and skin, we feel parched and dry; fevers and susceptibility to cold, together with other human ills, are manifested.

Who has not noticed the great access of disease and of lesser bodily disturbance which a dry heated term of our summers is sure to bring? These evils may all be observed in our furnace heated winter dwellings, and are making their impression particularly upon the rising generation of our cities. Dr. John Bell has said that we, "of the middle states, are nearly in the situation of those who should spend their summer in Egypt and their winter in Russia." Desor attributes the restless nervousness of Americans to the dryness of our climate.

We can overcome these difficulties, at least in the winter, by attending to the relative humidity of our dwellings, and we may constantly test their degree of humidity by the simple hygrometer of Edson or of Mason.

The problem to be solved falls into two parts:—

1st. What is the relative humidity for an agreeable temperature most conducive to health?

2. How may we maintain that humidity by a hot air furnace.

If we determine the amount of moisture needed in the air for health, we can ascertain very readily the quantity of water in the cold air, and consequently how much we must add to it to give it when warmed, the healthful relative humidity. It will be the duty then of the furnace (supported by a proper construction of the building for ventilation), to give this water.

The problem is a very difficult one for large halls filled with an audience, but for private dwellings is much simpler.

1. Experiments upon the first head are wanting.

We will not go astray, though, if we assume that the Creator has placed us to live in an external atmosphere of the proper relative humidity, and that since he has given us, through our reason, the power to combat the cold of winter, we may use the same faculty to draw proper conclusions as to atmospheric moisture.

The relative humidity of the external air varies from 100 to 12, and lower. It is greater in winter than in summer, although in winter there is less water in the air on account of the cold having condensed the moisture. The mean relative humidity of the year will give us that quality of air best suited to our nature, and that which we must imitate in our plans for artificial warming and ventilation.

The mean relative humidity of Washington for the years 1856–58–59, was 68·15.

That for Philadelphia from twelve years observation was 68·5. Müller gives the annual mean relative humidity of Halle, Germany, at 75. Roscoe states that the experience of heating and ventilating the House of Lords, demonstrated the most agreeable relative humidity to be not less than 55, nor more than 82, of which the mean is 68·5. My own observations of the air of the Mammoth Cave of Kentucky, showed a relative humidity of 87·6 and a temperature of 58°; an agreeable temperature and humidity for exercise.

These considerations lead us to adopt for the relative humidity of our winter dwellings the standard which nature furnishes us; it should range between 50 and 75, of which the mean is 67·5. It should be, as I have given it, slightly lower than the annual mean, since the relative humidity is a little higher in winter than in summer. The proper degree of humidity in our warmed rooms renders a lower temperature agreeable; the moisture in the air acts like a blanket to retain the heat of the body.

2. In order to maintain the relative humidity of the air of apartments at a given point, say only 50 for a temperature of 70° Fah., the temperature of the external atmosphere being at 32° Fah., and nearly saturated with moisture, we have to make calculations which will give varied results, according to the construction of the house. We must consider how much warm air per minute enters it from the furnace, and how much from the external atmosphere by the windows and doors, the sum of these leave it per minute, and constitute the ventilation. Also what are the number of its inmates, and how many lights are burned; although this is of minor importance in private dwellings, where the ventilation is generally in excess.

In a sleeping room in Washington, heated by a fire in an anthracite grate, and occupied by two adults and two children, the relative humidity in the morning before any doors were opened, was 46, the temperature of the room being 68·4 Fah. The carbonic acid was only double that existing in the external air, demonstrating a very efficient ventilation effected by the chimney.

The following calculations must be regarded as giving only general results to direct attention to the subject, and to form a basis upon which to experiment for the attainment of a more perfect furnace.

A cubic foot of air at 32° Fah., when saturated contains 2·126

grains of water. Let us suppose that it is nearly saturated, and contains 2 grains. If it be raised in temperature to 70°, its capacity for moisture is raised to nearly 8 grains, and as no water has been added, it is no longer nearly saturated, but is *very dry.* Let us see how much water it contains. Air expands $\frac{1}{492}$ of its volume for every degree above 32° Fah.; 492 cubic feet at 32° would become 530 cubic feet at 70°; hence, 1 would become 1·077 cubic feet. These would contain the two grains of water; hence 1 cubic foot would contain only 1·85 grains of water; *its relative humidity has been reduced by the warming from nearly* 100 *to* 23·11.

If we take the minimum relative humidity (viz. 50), that can be afforded for health, this cubic foot of air should contain 4 grains instead of 1·85 of water; there is therefore a deficiency of 2·15 grains for every cubic foot of air of the apartment. In a room of 15 × 20 × 12 feet, containing 3,600 cubic feet, this deficiency would equal 7,740 grains of water, or more than a pint (which equals 7291·11 grains), and in a house of eight times the capacity of the above room, the deficiency would be 61,920 grains, or a gallon plus half a pint.

Every time the air of the house is renewed, a gallon of water must be added to give its *minimum* relative humidity of 50.

The inmates of a room add water to it from their skin and lungs which, according to Hood, amounts to 12 grains per minute, and according to Seguin 18 grains. But carbonic acid is also added, and this must be removed by the ventilation. Hood estimates the ventilation for one inmate at $\frac{1}{4}$ cubic foot per minute for the carbonic acid, and $3\frac{1}{2}$ for the bodily moisture, say about 4 cubic feet per minute. If these 4 cubic feet per minute were not removed, in every hour, only 720 grains of water would have been added to the air from the person of the inmate of the room, leaving still 7,020 of water to be supplied to maintain the relative humidity at 50.

The non-removal of the products of respiration for one person in a room of 3,600 cubic feet capacity for an hour, upon the supposition that such person exhales 0·7 cubic feet of carbonic acid in that time, would give to the air 6 volumes per 10,000 of that deleterious gas; or two volumes more than the quantity naturally present in the air, which is four volumes. It would require $10\frac{3}{4}$ persons to give to the above room by the evolution of their bodily moisture the proper degree of relative humidity, and they would bring the proportion of carbonic acid up to 21·5 volumes per 10,000

(upon the supposition of no ventilation), a quantity entirely too high for health. It follows from this, that it would be impossible to rely upon the respiration and perspiration as a means of supplying a healthful moisture to the air, to say nothing of the disgusting fact of breathing again air thus vitiated.

The absolute amount of water to be supplied.

If we knew how often the air of a house, of the capacity cited, were changed we could easily calculate the water necessary to be added to maintain a relative humidity of 50.

Thus, if in our example it were changed every hour, a gallon and a pint of water must be added to it every hour.

If all of the fresh air came to the room through the register of the furnace, the problem would be less complex, for the proper amount of moisture could then be added at once in the hot air chamber; but a large portion of the air comes from the external atmosphere proceeding from the opening of doors, or drawn into the room through the cracks of the doors and windows. If there be a system of ventilating flues, the renewal of air may be regulated to a certain extent, but if there be open fire-places or fires burning in grates, the ventilation is frequently excessive, and more water of hydration is required per hour. In either case, a large portion of its fresh air enters the room by the cracks of the doors and windows. This obliges the furnace air to be hydrated to a *greater extent*, in order to maintain a relative humidity of 50 in the room. We must avoid the mishap of having such an excess of water that it is deposited in the liquid condition, especially when the heat is turned off from a larger to a smaller number of registers. Hence, to be perfect, an apparatus for hydration should admit of regulation. With respect to the rate of the renewal of the air, it has been found necessary in the halls of Congress to change it every eight minutes, *i. e.* seven and a half times each hour. This rate would not be required for an ordinary dwelling-house, nor could it be effected without powerful means; it would need the evaporation of nearly eight gallons of water per hour.

General Morin performed a series of experiments upon the ventilating effects of chimneys, and the crevices of windows and doors, upon an office in the Conservatoire des Arts et Metiers. (Expériences sur les effects de ventilation produits par les cheminées d'appartment Comptes Rendus LVI. p. 16, 1863.) The room could

be heated at will by an open fire, or by the register of a hot air furnace. He found that the chimney without fire ventilated the room abundantly. He measured the air entering the room, and that evacuated, by determining its velocity by means of a delicate wind wheel, and found that when the external air had a temperature between 35° and 50° Fah., and the interior temperature ranged from 64° to 71°, the chimney withdrew from the room, every minute, 235 cubic feet of air (more correctly 235½), of which 90·30 entered from the hot air furnace and 144·77 through the crevices of the windows and doors. Or about three-fifths of the air entered by the crevices and was warmed by the remaining two-fifths of air furnished by the furnace. The quantity of fresh air thus added every minute, would be contained in a cube, whose edge is a little more than six feet.

Applying these data to our hypothetical house, the air would be renewed every *quarter of an hour*, and would require four gallons and two pints of water to be furnished to the house every hour. Such ventilation is excessive; one-half, perhaps even one-quarter would probably be sufficient for an ordinary dwelling, which would require the addition of two gallons of water (or of one gallon), per hour. It would depend upon the ratio existing between the fresh air furnished by the registers, and that admitted to the house through the crevices, how much of this water could be added by evaporation in the hot air chamber of the furnace.

It is apparent from these considerations, that the hot air of the furnace must contain water almost to saturation in order to effect a healthful humidity, and that the small pan of water in furnaces is entirely inadequate to the purpose. The following experiment was performed in the investigation of the ventilation of the Capitol. An iron tank, containing sixty square feet of water surface, and warmed to 172° Fah. by steam pipes, was placed in the main hot air duct of the House of Representatives. A volume of hot air of the capacity of the House of Representatives passed over this tank every eight minutes, causing the evaporation of not quite fifteen gallons of water per hour; the increase of moisture in the air due to the tank was not more than half a grain per cubic foot of air. With a less active ventilation, the amount of water per cubic foot would have been somewhat greater; but it follows from the experiment that the rapid passage of air over a heated surface of water is not sufficient for its hydration. It is necessary to furnish the water more rapidly as by boiling it; I should have liked to have kept the water in this tank in a state of rapid ebullition.

Suggestions for an improved furnace.

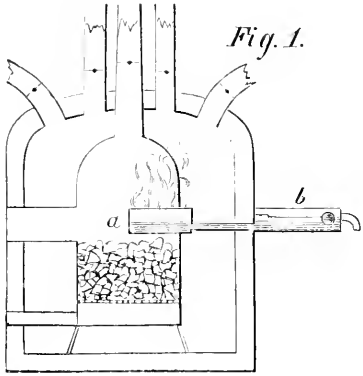


Fig. 1.

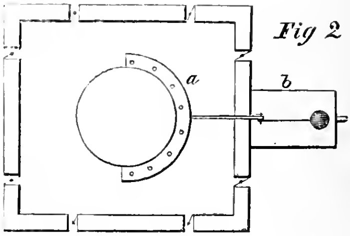


Fig 2.

The following suggestions are offered to practical men for an improvement in furnaces.

1. Cease improvements of the stove, making it more complicated and expensive and saving both coal and the trouble of charging it, until a good water supply capable of being regulated is obtained. This would not prevent the supply of moisture to any such complex furnaces if they be preferred.

2. Save the coal, as is done, by dampers in the fire flue and register, in the ash-pit and stove-door. Also by attention to the quantity of air passing through the hot air chamber governing it by dampers in the hot air ducts near the stove, and in the cold air ducts; paying also great attention to the air leaving the apartments, and regulating it as much as possible.

3. Seek some method of adding steam by the action of the fire, and in regulated quantities under control, to the hot air chamber. Perhaps the water tromp for obtaining a blast by the fall of water or the atomizer may be made available. Perhaps some means for the evaporation or atomization of water in the apartment may render assistance in maintaining the proper relative humidity.

The admission of steam to the hot air chamber does not rest upon theory only; it has been carried out in practice by Professor Henry at his dwelling in the Smithsonian Institution. "An iron tube connected with the water vessel in the hot air chamber, was inserted through the side of the furnace into the midst of the burning fuel;" this device kept the water in the vessel in a state of rapid ebullition, raised the relative humidity of his apartments, and a quality

of "softness and salubrity were imparted not before perceived" in the air.

4. The above sketch is suggested, in which Fig. 1 represents a vertical, and Fig. 2, a horizontal section of a furnace improved for hydration. The supply tank, with ball cock, may be placed inside of the air chamber, providing for an overflow through the wall. The water back should not be in the fuel, and devices should be present to prevent unpleasant results from derangement of the water apparatus.

The water must *boil* at the rate of a gallon, or more, per hour according to the ventilation, for a moderate dwelling.

THE NATIONAL WATCH COMPANY.

By Prof. S. W Robinson, C. E.

On passing through the pleasant little city of Elgin, Illinois, a few weeks since, I ventured to sacrifice a few hours of time at that part of my journey, with a view to visiting the *National Watch Company's Works*, located at that place, to catch a glimpse, if possible, of those wonderful machines that turn out the tiny wheels, which, by their undeviating movements in their allotted spheres, measure off so truthfully the few fleeting moments of human existence. The buildings were reached by about half a mile's walk from the depot. They were originally located at some distance from other buildings and streets, to avoid dust and confusion, but the hundreds of operatives who have come to populate the working rooms of the establishment, and furnish brains to the machines, are building up houses in the vicinity, so that the isolation of the buildings is not so complete as formerly. They consist of a main part, about 40 feet square and three stories high, with which are connected three wings; those toward the east and west being 100 by 28 feet, and two stories high. The south wing, 87 by 28 feet, has one secondary wing, 25 by 35 feet, extending eastward, and the engine room, 30 by 65 feet, on the west side. All are substantial structures, consisting of brick and stone. On entering the janitor's room, and furnishing to one of the proprietors, called for the purpose, sufficient assurance of unobjectionable motives, ready access was granted to the various rooms and departments.

The machine shop of the factory is situated in the lower story of the west wing. The other rooms of the buildings, excepting the east wing, are crammed with the watch-producing machines. About three hundred persons are here at present employed, producing about fifty watches per day. This falls so far short of supplying the great and rapidly increasing demand, that the east wing, lately finished, is being rapidly fitted up with machinery, and the works extended into it. By this addition to the present facilities of the Company, it is expected that, within two or three months, their producing capacity will be increased to nearly one hundred watches per day. This large and thriving trade is being protected by a sound and vigorous policy. No watches are supplied at the factory to individuals, for any consideration whatever, much less at a figure below that of the dealer. They are only furnished at regular fixed rates, and in a considerable quantity at a time, to those in the trade; so that they can have the satisfaction of retailing them at the very lowest prices for which it is possible to secure them. No cases are now made by the Company. The tradesman can therefore supply himself from whatever source, and with whatever quality he chooses.

The machines used in this establishment were manufactured by the Company in their machine shop, occupying a portion of the west wing. Only a few of these machines are automatic, so that the greater portion of them require, separately, an attendant. They are, therefore, much less complex and expensive. As their management requires light and quick movements, they are, perhaps, better manipulated, as in fact they mostly are, by the delicate fingers of girls, who stand ready, by hundreds, to work them at rather low wages. So long as help is cheap, the temptation to complicate the machines into automatic ones, with the sole object of reducing the number of attendants employed, is not very great. Indeed, many of the machines are so simple that they may almost be regarded as only tools, to serve as undeviating guides to the erring hand, enabling it to "draw the bead" so closely upon the fine steel wire. The machines, therefore, exhibit a measure of ingenuity in being simply the very thing needful to the hand of the operative, enabling him or her, with only a few weeks' or even days' practice, to turn out thousands after thousands of separate pieces, so nearly alike that any one of them will satisfy the test of delicate calipers, by which the different thicknesses of two fine hairs may be deter-

mined with the greatest facility. Instead of adopting such simple aids as these, the trans-atlantic watchmakers must devote a lifetime to the acquirement of accuracy in handicraft, transforming them into human machines, who even then can but produce imperfeet work, such as requires to be set up by the old "scribe rule." But here, as in all other great manufactories of this country, any piece will fit in the place of any other similar piece, in any article of its kind ever produced.

Of the watches produced by this Company, there are six different grades now in the market, varying largely in price and considerably in quality. The six grades bear the following trade marks, viz:

1st. B. W. Raymond,	4th. G. M. Wheeler,
2d. H. Z. Culver,	5th. Mat. Laflin,
3d. Culver,	6th. J. T. Ryerson.

The essential difference between the first and last three grades consist chiefly in the former having finer toothed wheels, by which the impulses communicated from the main-spring to the escapement are more uniform and regular. The "B. W. Raymond," which is placed in the front rank, in honor of the president of the company, receives numerous fine strokes of the file and polishing powder, whereby some of the corners that appear under the microscope are removed from the minute parts. Although this contributes to their infinitesimal elegance and beauty, it cannot warrant us in claiming therefor much better performance in the pocket. Partiality is also shown this watch in selecting for it the most perfect pieces from all the three first grades. The second best selections are allotted to the next grade. The remainder is appropriated to the last of the first three grades. Although the last three grades are distinguished from the first by having coarser teeth in their trains of gearing, still the difference is not very great, the ratio numbers being about as 8 to 10. The different qualities are here determined as in the first three grades by the method of selections. But these selections do not give rise to so great a variety in quality as one might suppose. Every piece being turned out with the greatest accuracy attainable by practice with machines, giving results which satisfy the most delicate tests, if even one of the lowest grades is proffered us, we need not, as we are amply assured by competent judges, look further for greater perfection in foreign productions. The only superiority possessed by even the highest priced European pocket-pieces over the American, as now

found in the market, consists in the greater care bestowed upon the adjustment of the escapement to pure isochronism. This correction alone is sufficient to run up the cost of a watch indefinitely. It requires the selection of a hair-spring that will give exactly equal times of vibration, whatever may be the extent of vibratory arc. The detection of such a spring, in any given case, is almost a matter of pure chance, and can only be effected by patient and repeated trials. A timepiece thus adjusted is also a most delicate instrument, and should be handled with the most profound respect for its refined virtues, lest at any time they should suddenly depart. Any binding or straining of the spring, while in the hand of a bungler, would greatly jeopardize their safety. The first and chief condition of isochronous vibration is, that the tension of the spring shall increase uniformly with the arc of vibration from the point of repose. As any good spring will very nearly fulfil this condition, and as the immediate object of our American companies is to supply a good substantial article at a low figure, already performing with almost wonderful accuracy without introducing that effeminate principle of unwarranted permanency, which would easily increase the cost from \$50, the price of the "B. W. Raymond," to from \$100 to \$200, without adding a single particle, should we express much surprise that this delicate adjustment should be reserved for the higher priced watches and chronometers?

The escapement introduced into the "National Watch" is of the lever stamp, although called the "straight line escapement." The pallets are "exposed," and situated in a line nearly perpendicular to the lever, which together resemble an anchor with the shank turned outward. The plain steel balance is not so much used as the expansion balance, the latter being preferred by the Company for reputation's sake, and of course preferred by the purchaser. The expansion balance in the higher priced watches is corrected, by trial, to compensate for heat and cold. The very best materials that can possibly be obtained are used in the manufacture of these watches. The pinions, and other steel parts, are made of the best Stubs steel, well tempered and polished. The jewels are of garnet, ruby, and other precious stones, so hard that they will easily scratch glass, and can only be worked by the use of powdered diamonds.

Among the advantages claimed for this watch over all others are the finer wrought gearing and quicker beat. The number

of teeth in the trains of the "Appleton, Tracy & Co." of the Waltham Company, lies between those of the first three and last three grades of the National Watch Company, but most nearly agrees with those of the "G. M. Wheeler," and the following. The Waltham watch has a quick beat, making 16,200 single vibrations per hour. But all those who ride upon railway trains, where the watch is subject to sudden jerks and jars, will at once recognize the advantage of increasing this number to 18,000 per hour, as is actually done in the "National Watch." Although the Waltham watch has, and still is enjoying a high reputation, and has gone into most successful competition with those of European make, yet may we not now unmistakably adopt the concurrent testimony of those who have tried the different timers, and then agreeably look to the Northwest for our real *National Watch*, and feel more than ever the truthfulness of the poetic conception, "Westward the star of empire takes its way."

University of Michigan.

ANALYSIS OF A NEW PIGMENT.

By Chas. P. Williams.

(Late Professor Analytical Chemistry, Polytechnic College.)

The material from which this pigment—now largely sold in New York—is manufactured, is the mixture of zinc blende and galena from the washing buddles of the Silver Hill Mine, Davidson County, N. C. The activity of this mine (since the commencement of mining operations in 1838, under the name of the Washington Mine) has been somewhat intermittent, but when in operation the main object of its exploitation has been the silver and gold occurring in the galena which is associated in the vein with a large amount of blende, and with some iron and copper pyrites. The process of concentrating the galena for the smelting works by means of the circular buddles has accumulated several thousand tons of blende with a small per centange of galena, which, within the last eighteen months, have been utilized by a preliminary roasting at the mine, and by a subsequent treatment at Bergen Port, N. J., according to the method ordinarily pursued in this country for the manufacture of "zinc white" or oxide of zinc. The product thus obtained is, chemically speaking, an impure oxide of zinc, containing a considerable amount of sulphate of lead, and somewhat resembles in com-

position the condensed "fume" from lead furnaces in which a galena containing some zinc blende is treated.*

The following are the results of some analyses recently made of an average sample obtained from a barrel of the dry material. No. I. is by myself; No. II. by Dr. G. A. König, in my laboratory:

		I.	II.
Insoluble in water.	Oxide zinc	72·083	71·680
	Oxide iron	trace.	trace.
	Oxide antimony	trace.	trace.
	Sulphate lead	23·968	24·610
	Oxide lead	·274	not estimated.
Soluble in water.	Sulphate zinc	·810	·931
	Chloride zinc	·839	·965
	Chloride iron	·071	not estimated.
	Chloride antimony	trace.	....
	Chloride cadmium	·256	·191
	Moisture at 212° Fahr	·398	·270
		98·699	98·647

The method produced in these analyses consisted in extracting the samples with *cold water*, and estimating the chlorine, sulphuric acid, etc., in the usual manner. The insoluble residue was then treated with a solution of hyposulphite of soda till the filtrate no longer gave the lead reaction with sulphydric acid. By this the sulphate of lead was dissolved out, and was afterwards precipitated by sulphydric acid gas, the resulting sulphide of lead being reconverted into sulphate of lead, and weighed as such. The residue was dissolved in nitric acid and was found to contain a small amount of oxide of lead, the oxide of zinc, and traces of oxide of antimony and oxide of iron.

This plan, though somewhat tedious, was followed to avoid as far as possible, all theoretical considerations in reference to the conditions of combination in which the various substances existed in the material.

* Compare Kerl's "Handbuch der Huttenkunde," Vol. II., and Watt's "Dict. Chemistry," Vol. III., p. 526, for composition of "lead smoke" from various processes.

Philadelphia, March 2d, 1869.

EDUCATIONAL

LECTURES ON VENTILATION.

By Lewis W. Leeds.

Second Course, delivered before the Franklin Institute, during the winter of 1867–68.

LECTURE III.

(Continued from page 138).

Heat is the subject for consideration this evening. Fresh air is the greatest of blessings, and to supply it, ventilation is, as we have shown, of the first importance; yet we are much more sensitive to heat and cold than to *pure* or *foul* air.

Heat we have to furnish, in a great measure, ourselves: pure air is supplied us by the Creator. Heat is the prime moving power of the air: it is also the great controlling power of ventilation. It is the *improper cold draughts* that make ventilation so unpopular.

Heat, therefore, is the key that unlocks the door for the free flow of pure air, *i. e.*, for good ventilation. Hence, if we desire such ventilation, we must secure the key that unlocks the door where the treasure is kept. And now let us be sure to secure the *right* key.

There are three distinct methods of heating:

1st. *Radiation*—in which the pure, independent streams, called rays of heat, come directly from the sun or other hot bodies.

2d. *Conduction*—or the means of conveying heat through solids, when the particles of the heat are handed from atom to atom. And

3d. *Convection*—which is the condition wherein certain moving bodies, such as air, water, or other fluids and gases, gather up an armful of heat, as it were, and carry it away in their arms to other parts.

Now, it is necessary that we fully and clearly comprehend these three different ways of communicating heat. It will require a little close attention, but the understanding of it is not difficult, and then

the whole subject of heating and ventilation, which may now appear as a mystery, will soon begin to look quite plain and simple.

For the purpose of classifying the different kinds of heat more distinctly in our minds, let us term them according to their relative value, as follows:

Radiant or *Golden* heat;

Conducted or *Silver* heat; and

Convected or *Copper* heat.

This is about the relative value I put upon these different ways of communicating warmth, for our practical purposes. Now let us consider these three several ways of heating, a little more in detail, taking first radiant heat, or our "gold heat," as that is much the most valuable.

There is a tendency to a perfect equalization of that sensation we call heat, in all bodies. If you should bring a block of ice into this room, and a red hot cannon ball, in a little while they would both have become of the same temperature as the room; the ice may have changed form and turned into water, but (no matter for that now,) they would both have become, as I said before, precisely of the one temperature with that in the room.

The rays of heat—like the rays of light from the candle which I have here—are thrown in every direction equally, just as much downwards as upwards, and just as much to the right as to the left.

The special point of interest to us is the effect that the radiant heat has on the air surrounding us. I want you to comprehend clearly that it has *no effect*—it has no direct influence whatever in heating the air. The understanding of this is of great practical value in our study of the subject of heating and ventilation.

This room has air in it, but it is not full, because, by properly pressing it down, we could put in it the air of another room of similar size, and by pressing it more we could add another, and another, and so on until we had got one hundred times as much in it as it at present holds; therefore, we must know that it is not the one-hundredth part full. But now again, if we were to commence at the top, supposing it to be air-tight, of course, and were to remove ninety-nine hundredths of all the air in the room, the remaining one-hundreth part would be evenly distributed over the whole space, so that there would be just as much at the top as at the bottom.

There must, therefore, be some *other* substance or property in the

room besides air, and which has the power of separating the particles of air and keeping them equally apart from each other. And herein we see exemplified another beautiful provision for the universal distribution of the air; so that should man, or any living animal, happen to be confined in a partial vacuum, yet all the air that is there would be equally distributed throughout that space.

Now, suppose we compress our ordinary atmosphere one hundred times; still the individual particles do not touch each other, as otherwise it would become a heavy solid, though it would then only weigh about ten pounds per cubic foot, whereas the same measure of gold weighs more than one thousand pounds. Hence, each individual particle of air must be separated a great many times its own diameter from its nearest neighbor—perhaps, one hundred, or it may be, one thousand times—for any determinate number, in the present state of our knowledge, would be purely conjectural.

Imagine a regiment of soldiers drawn up in line of battle, but instead of standing close together, they were to be posted a thousand times their own diameter, or a quarter of a mile, away from each other—do you not see how many of the enemy's bullets might pass through the line without hitting any one? These individual particles of air, therefore, are so far apart that there is very little probability of the rays of the sun, or the rays from any other hot body, striking them; and I believe it is considered that they are ray-proof, even against the few rays that might happen to hit them.

Though I cannot illustrate this by showing you radiant heat, yet the rays of light were very similar, and I will endeavor to make the effect evident to you by a simple experiment with this candle. I simply place around the candle an opaque cylinder that interrupts the rays of light that are going directly towards you. But why do you not see the column of rays that is passing directly upwards, and which you can see by that small bright spot on the ceiling? To make it more distinct, I will move the light about a little, and you observe the spot of light moves correspondingly.

Now, the *reason* you cannot see that column of light is because the rays move directly on through the intervening space, having nothing to stop them until they get to the ceiling. I say "nothing," because the air is so near nothing that it allows of no perceptible effect; but now let us interpose something that will stop those rays—this piece of paper, for instance, will collect them, by which

the disc of light becomes quite visible—and, of course, being thereby obstructed, they cannot go on to the ceiling. Or, by holding this smoking taper in the path of the rays, there is sufficient solid material in the smoke to obstruct a large portion of those rays, and the column thus illuminated is no doubt plainly visible to all.

So you cannot see the rays of *heat*, but they are passing through the same space, and are very similar, in every way, to the rays of light, the difference in the effect produced being considered due to the difference of velocity with which they move.

That smoke will become *heated* as well as lighted:

Experiments have proved that perfectly pure air, and the gases—oxygen, hydrogen and nitrogen—do not absorb an appreciable amount of the sun's rays, and perhaps if they could be made entirely pure, they might be found to be absolutely ray-proof. But you know that the moisture absorbs a great amount of the heat, because a cloud passing over us of a hot summer's day is an excellent shield from the burning rays of the sun; and there is a large amount of moisture in the air at all times, even when not seen in the shape of clouds.

But you may say, we know that the air does get heated—how is it?

We must again set our imagination to work; we must endeavor to see with our mind's eye those little particles of air, which we at first supposed to be kept constantly separated, and so far apart as scarcely to get within sight of each other, but which, when we come to examine, we find are really clustered together in little groups or families. This we know from the fact, that where we find one atom of oxygen (in the atmosphere), we are sure to find with it four atoms of nitrogen. And again, we find that where these air particles become heated, they are expanded, therefore we must conclude that they are associated together, as just remarked, in little groups or families.

Now, the sun's rays pass through the forty-five miles of atmosphere without heating it, but when they strike the solid substances on the earth's surface the temperatures of the latter are very soon raised. We know this by putting our hand on a board fence, or wall, when the sun is shining on it, or on the curtains or blinds inside of the glass window, which become very much heated by the solar rays.

Last winter I placed a thermometer in a box, lined with black

cloth, and having put a glass over it to keep the wind off, placed it in a snow bank: the mercury soon rose to 180°. We can boil water by the sun's rays even in winter.

When the little groups or families, before mentioned, come in immediate contact with a hot substance, the heat is transmitted to them *by conduction*—each group, so to speak, receiving its "armful" or complement of heat. Being expanded thereby, they are made lighter and forced upward, whilst other particles rush in to take their place; and these latter, as they strike against the hot body, are in turn heated and caused to rise, so that a continuous stream or current is thus created.

As this will be so constantly referred to in our examinations of the heating and ventilation of buildings, and as we ought to be able to comprehend the principle so as to apply it for the regulation of our own health and comfort every hour of the day, I hope you will fully realize that there are two distinct forces constantly acting in opposite directions near every hot fire or other hot substance. One is, the rays of heat passing in every direction *away* from the fire; and the other is the flow of cold air *towards* that fire, and directly in opposition to the rays of heat.

But, that air is not heated at all until it strikes against the hot body itself. The most philosophical way of roasting a turkey, therefore, is to hang it up in front of a hot fire, and use the golden rays of heat to cook it, while the cold, pure, concentrated air is flowing by it into the fire.

Conducted heat has less influence upon us, perhaps, than either of the other systems. As we are affected in this way only by those things that come in immediate contact with us, of course the better or worse conducting power of our clothing has much to do with our comfort.

But *convected* heat is the great curse of the American people. It is that dry, lifeless, withering, debilitating, poisoned stuff with which most of our best houses and public buildings, and, most unfortunately, many of our school houses, too, are filled and warmed; and which is filling our systems, and warming and drying all the life and substance out of about two-thirds of the people of this country.

I have, for the last three or four years, been giving very close attention to the comparative merits of radiant and convected heat. I formerly supposed that air warmed by a hot water apparatus

was as perfect as any artificial arrangement could be, but more careful recent investigations have shown me my great mistake. Let me first say, however, that hot water is probably the most perfect means of warming what *air* we *must* have warmed.

But what I mean to express, particularly, is, that we should be surrounded with and breathe *cold air*, and receive the *heat* required for the body by direct radiation.

This is just the reverse of what we generally experience in our furnace-heated houses. There we have a little hot, dry air—the air itself being hotter than the temperature of the room, and the walls, especially the windows, colder. No more unscientific, unhealthy and uncomfortable condition could possibly be imagined than this.

Our Creator has provided for a constant difference between the temperature of our bodies and the air with which we are surrounded, for a wise purpose; and we find the greater that difference, the more active and vigorous is the exercise of all our animal functions. As I stated the other evening, there is nearly double the quantity of carbonic acid given off in a temperature near the freezing point that there is in a temperature of °90 or °95.

From this we would argue that when we are breathing air which is near the freezing point, we are twice as active, and can do twice as much work, as when we are breathing air of nearly the temperature of our own bodies. We know how very languid and good-for-nothing we feel on a hot summer's day; and, on the contrary, how fresh and vigorous we feel in the open air of a cold, bracing winter's day, and especially when the glorious golden rays of sunshine come pouring down upon us with all their richness and splendor.

(To be continued.)

SUNLIGHT AND MOONLIGHT.

A Lecture delivered at the Academy of Music, before the Franklin Institute, on May 23d and June 6th, 1868.

By Prof. Henry Morton, Ph.D.

(Continued from p. 135.)

We know of no way in which, so satisfactorily, an idea as to the usual character and behavior of sun spots can be given, as by the detailed description of a characteristic specimen, such as was published by the Rev. Mr. Howlett, in the Monthly Notices of the

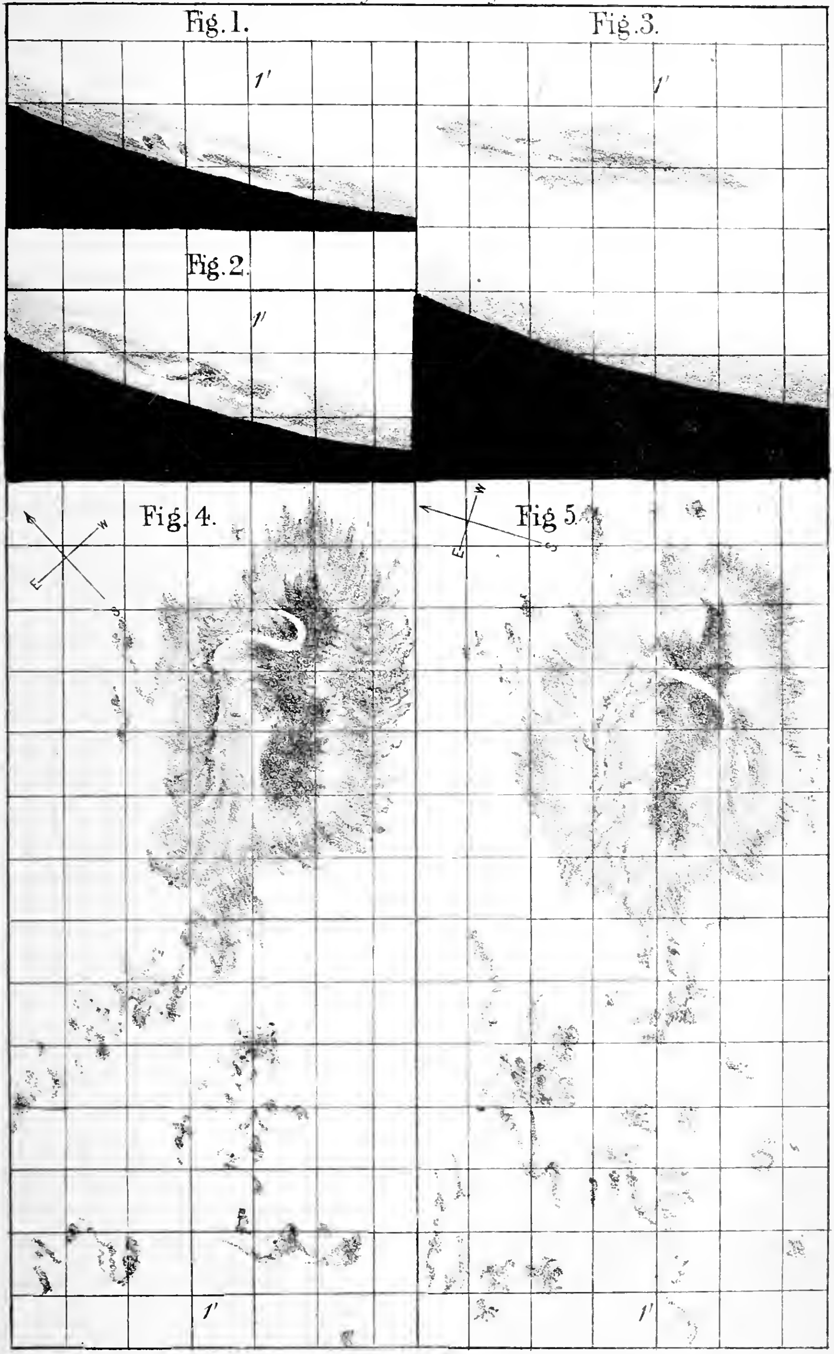


Fig. 1.
Fig. 3.
1'
1'
Fig. 2.
1'
Fig. 4.
W
E
S
Fig 5.
W
E
S
1'
1'

Royal Astronomical Society, November 10th, 1865, Vol. XXVI., p. 13.

We will quote from the description of this eminent astronomer, such passages as will explain the accompanying drawings of a spot which appeared in October, 1865:

"It was first observed by me on the morning of the 7th October last, at 7 A. M., and when not more than about 3″ from the Sun's eastern margin (Fig. 1). It then appeared about 57″ in length from N. to S.; but, in consequence of the extreme effects of foreshortening on the surface of the sphere, only 3″ in breadth from E. to W.

Narrow though the spot, therefore, at this time appeared, yet the positions of the umbræ within it were distinctly visible, and distintinguishable from the penumbra in directions of latitude, though I was unable to discern clearly between umbra and penumbra in directions of longitude.

One feature which it struck me as certainly not often to be seen at such an *early* stage of a spot's entry on the disk, was the appearance of two or three luminous patches of faculæ (immediately adjoining the spot on its following side), which extended *completely up to the very margin* of the Sun. * * *

By 2h. 15m. P. M. on the same day (October 7th), the apparent mean breadth of the spot had attained about 7″, and the distance from the limb to the centre of the principal (or western) umbra was 13″ (Fig. 2).

The distinction between umbra and penumbra was now in all parts discernible; and I had at this hour more satisfactory evidence of the *shelving* nature of the sides of a Solar spot than I think ever occurred to me before; for the penumbra on the *following* or *outer* side of the umbra was plainly broader by about 3″ than it was on the preceding or inner side, on which last, in fact, it could not have been much more than 1″ in apparent breadth.

So shallow, however, is the amount always of this shelving, at least in the penumbral strata, and so seldom, comparatively, have I enjoyed the opportunity of observing the entry or departure of a really good-sized spot, furnished at the same time with a neat and really *centrally situated* umbra, that it has only been on two or three occasions, during my six years' experience, that I can fairly assert that I have seen this difference of fore-shortening at all. * * *

On the morning of October 8th the spot had attained a length of

about 76″ by an apparent breadth of about 16″. Its centre was now about 52″ from the Sun's limb; and the penumbra appeared equally wide on both the preceding and following sides of the principal umbra, which lay within the southern portion of the spot (see Fig. 3). * * * *

October 13*th* (Fig. 3). The group this day had attained its most central position on the disk; and as regards size, also, had now reached its utmost dimensions, being about 110″ in length by 60″ still in breadth; and, making every allowance for its oval and also somewhat irregular contour, must have had a superficial area of certainly not less than 972,000,000 square miles. The remaining small spots had a joint area of about 165,000,000 miles; making a grand total of displacement of the solar photosphere to the enormous extent of 1,137,000,000 miles square, or nearly six times that of the whole surface of the earth!

The penumbra on the morning of the 13th was marked by a long dark streak in its northern portion, about 40″ in length and only about 2″ in breadth, which by noon had become more dark and distinct, as if about to become a narrow umbral *rift;* and other shorter streaks lay in nearly parallel lines with it, towards the south-east; whilst another streak from its north-west extremity ran at right angles into the northern side of the umbra, and was divided across by a small bright patch at the hour last mentioned; by which time also a large square projecting portion of penumbral matter had nearly detached itself from the main spot towards the south-east.

But the most remarkable feature, perhaps, this day, was a bright, sharply-defined arch of photospheric matter, about 9450 miles long in its total curve, which floated over the western side of the umbra, and was united at each extremity to the northern side of the penumbra.

I was now also able, as I thought, to discern four separate nuclei in different parts of the great elongated umbra, which feature itself was not less than 26,000 miles in length by 15,000 in breadth; and wherein, towards the south-east, a large triangular mass of the feebly luminous haze was still plainly observable. It is, perhaps, possible that the arch above described was constructed by the union of one of the promontories with the remains of the sigmatoid extension which existed, as we have seen, on October 12th.

By 8 A. M. on October 14th, the great spot presented a very dif-

ferent appearance (Fig. 4). The umbra was completely divided across into two unequal portions by an exceeding bright and nearly straight bridge, 9000 miles long and 1000 miles wide, as before, and which seemed to be formed in part, possibly, out of a modification of the north-west remains of the arch of the day previous. It lay exactly over the principal nucleus of the great umbra, and was considerably dilated at its northern foot. But whether it was that the arch had swung itself loose at its north-east extremity, and then subsequently stretched itself out across the whole width of the umbra, or whether it was an entirely new extension of photospheric matter, I was unable to determine; but subsequent observations have led me to suspect rather the former. * * *

The rift, moreover, was divided by the luminous matter in two or three places, previous to its gradual closing up, as it afterwards proved. Equally curious were certain faint but perhaps not the less important features observable at this time amongst the small subordinate spots that followed in the wake of the principal one. These were evidently diminishing in magnitude, but in their manner of closing up they seemed to betray a noteworthy sort of movement among themselves, or perhaps rather, in the first instance, in the photosphere in which they lay scattered. The largest of these minor spots (which was of the dimensions of about 20″ by 15″) had either drifted away from, or had been left behind by, its principal; whilst at the same time a very peculiar sort of *trailing* arrangement was being assumed amongst themselves, indicating a kind of gyratory movement in the photosphere itself. Many of the little spots had ceased to exhibit any appearance of umbra, but, on the other hand, they had become united more intimately one with the other, by means of a wavy and here and there divaricating thread of penumbral specs running through them; an arrangement this, which may also be distinctly observed in my records, for instance, of 11th May, 1863, and of 1st February, 1864.

On the 15th October I was unable to make a detailed drawing of the group, but I observed distinctly that the great bridge had again become thoroughly curvilinear in its disposition, extending also further up towards the western end of the umbra, and having a total length of a clear 30″ or 13,500 miles. Indeed, in its now modified condition, and as being almost separated, in its new north-eastern portions, from the adjoining penumbra, it might almost be stated as being even 48″, or 21,600 miles long. The rift was also

still plainly to be distinguished, but its more easterly portions had nearly closed up."

In the further history of this spot, until its disappearance round the further limb on the 19th, there is nothing of special interest except that on the 17th the umbra was completely divided by a large mass of penumbra dotted with dusky portions. As is often the case, after the spot had passed out of view, there was seen (Oct. 20th) a conspicuous and dense mass of faculæ, very close to the sun's limb at the point where this spot had disappeared.

On the 3d of November a double spot appeared on the advancing edge of the sun, in the same latitude occupied by the spot just described, and being without doubt the remains of the same, brought round again into view by the solar rotation.

The impression produced by such a series of actions as we have just described, is certainly favorable to the hypothesis that these phenomena are due to displacements of the solar photosphere by violent aerial currents analogous to our terrestrial cyclones accompanied by formation and dissipation of the cloudy matter of the photosphere, such as would be natural under the variations of local temperature which must accompany such action.

Observations of Balfour Stewart and others on the predominance of faculæ on the following side of spots, seem to indicate an upward rush, as gas pushing out the photospheric cloud matter, while, on the other hand, the observations of Lockyer, Father Secchi and others on the shape of the penumbral markings at their edge, &c., seem to indicate an indraught or downward current towards the centre of the spot. May not these observations be reconciled on the assumption that a violent uprush of gas, almost like an explosion, opens out the spot, and that then a subsequent cooling and contraction follows with the production of an inward and downward current, which then continues with a rotary motion as in our whirlwinds and waterspouts? The subject is one of great difficulty and replete with discordant observations, which no theory which has been as yet suggested seems satisfactorily to reconcile. So thoroughly is this the case, that Lockyer, one of the most diligent students of this subject, in his late book on astronomy, declines to give any theory at all.

(To be continued.)

ON THE FUTURE DEVELOPMENT OF SCIENTIFIC EDUCATION IN AMERICA.

By S. Edward Warren, C. E.

Prof. of Descriptive Geometry, &c., in the Rensselaer Pol. Inst., Troy, N. Y.

(Continued from Vol. LVI., page 284.)

In contemplating the total field of study, we make the first and grandest distinction to be that of Creature and Creator, a distinction whose truth is sufficiently testified to by the universality of religion in some form.

The creature, so far as spiritual and uncorrupt, can know the Creator through His works, through its own intuitions, and through such communications as the Author of a spirit can, and, as is most natural to suppose, would make to that spirit.

In limiting ourselves, now, to the created universe, we distinguish, for practical purposes, Man and Nature; or, Man and all else that is created.

Man, being conscious both of self and the world, as separate, can make each an object of study.

Each, considered as free, or as uninterfered with by man, is properly said to be studied by *observation.*

Each, as conditioned by human interposition, is said to be studied by *experiment.*

Observation, then, we repeat, is the study of humanly free man and nature.

Experiment is the study of humanly conditioned man or nature.

The results of both observation and experiment, upon both man and nature, may then further be submitted to the exercise of reflection, or may be studied by reflection.

Now, first, in observation of nature, attention is given to the *perceptions.* In observation of self, attention is given to *consciousness.* In experiment, the *imagination* is exercised in devising the conditions to be imposed upon the objects of study. In *reflection*, the powers of *abstraction*, *judgment* and *reasoning* are exercised upon things seen or *remembered*, and the processes of comparison, classification, and generalization are conducted. Thus, in observation, experiment and reflection, the whole *intellectual* being is exercised; and if we include the stimulating motives, and inspiring ends which induce these activities, and remember the body as the instrument

of the spirit, the whole being, without qualification, may be said to be called into action.

SECOND.—Man and nature must be either free or conditioned. There is no third state possible to either.

Therefore, as observation, experiment, and reflection engage the whole being of man upon both himself and Nature, we might conclude that they are the sources of all original human knowledge whatever.

But the guiding distinction at this point is, that Nature and Life are not conditioned by man alone. They are conditioned for man by the Creator, so that the conduct of life presents itself as a series of practical problems for active solution. In other words, in all rational activity, or intelligent practice in the business of life, one is studying, experimentally, the right use of himself under the conditions in which he *finds*, not puts himself; how to develop and wield his powers and opportunities. But, in this form of experiment, the conditions are externally imposed, presented to the man, and not imposed by him. We have, then, an important difference of meaning between the expressions, learning by *experiment*, and by *experience*, or practice.

Whence we conclude that a perfectly composed course of study should provide for study by observation, experiment, reflection and practice.

But of these, reflection is chief, so that the results of observation, experiment, and life experience serve to fully develop or educate the man just in proportion as they are made food for reflection.

This fact demonstrates the impossibility of ranging certain studies exclusively under each head, and thus illustrates the principle mentioned before, that things are distinguished not by the *exclusive* presence of certain attributes, different in each, but by their predominance. Thus, the study of natural history, in its actual specimens, is conspicuously a study by *observation*. But it is by reflection upon the results of observation, and by repeated experimental attempts, that its objects can be properly classified. And again, all those special forms of a general conclusion in geometry, which result from particular suppositions on the given conditions, and which can be determined by inspection, are thereby really learned by *observation*. Also, it is by reflection upon experiments or experience, that the laws of physics and of animal or rational life are determined.

The principles just stated explain why it is that *any* study, thoroughly and fully pursued, can liberally develop the whole man. It is because when, in the familiar expression of *feeling*, we say a man throws his whole being, or goes with his whole soul, into any pursuit, he also does so *literally*—that is, he exercises all his powers on such pursuit, and therefore does broadly, generously, and truly educate himself by it. This also explains a former statement, viz., that the mind thrives best on what it constitutionally likes best.

Nevertheless, and especially when the natural bent of a mind is not very strong, and it seeks, accordingly, development by a prescribed course, such a course may well be composed of such a selection of subjects, as generally and most readily and obviously tends to cultivate each of the several powers of the mind.

We are now prepared to state the idea of truly liberal education. It should be one which, at every stage, affords, 1st, opportunity for study by observation, experiment, reflection, practice; and, 2d, provides various subjects, or exercises, under each head, from which the student can make a selection.

There is, however, an important limitation here to be noted. The world has seen two great civilizations—that is, two which are still visibly operative—the pagan civilization of Greece and Rome, and the modern civilization of the less corrupted portions of Christendom. A large body of men are personally interested, by education and association, in perpetuating systems of education based on the fruits of the former civilization. Another large body of men, now on the stage, and increasing in numbers, think it strange if Christian civilization cannot afford, in the study of free governments, of the broad political economy of the present, of the history, oratory, letters, and noble deeds, and the sciences of modern times, at least as good means of mental development as pagan civilization can, brooded over, as it was, by a crew of disreputable divinities. They claim that it can afford as good or better means. While, therefore, they respect all that is good in the achievements of the past, and would have the knowledge of them, and the culture based upon them, perpetuated so far as is useful in guiding and moulding the present, they believe that systems of education, based upon the two civilizations respectively, are best kept separate; and that, accordingly, it is a mistake to attempt to engraft schools of professional training in modern scientific pursuits upon classical colleges, as is done in several places in this country.

Hardly anything, perhaps, could more effectually defeat the very desirable end of securing graduates of a previous *general* or collegiate course, as students in such professional schools; since there is so great a want of adaptation in a classical course, as a system of preparatory *general* culture, for the *professional* study of modern science.

The last idea suggests one more which we must mention. The idea of a grand all-embracing unity besets and sways many minds. We confess to having once sympathized, more than now, with the idea of an encyclopædic institution, in which everything under the heavens should be taught; and this in behalf of a love for a visible unity. But there can be material unity—that is, essential unity of spirit, purpose, and effort, without formal unity—*i. e.*, uniformity of methods, and concentration of men and means into a single institution. But, nevertheless, formal unity of *some* kind is desirable, and it can be abundantly had by the simple expedient, so harmonious with the usages of the times in other departments of life, of mutual fraternal recognition among all higher institutions, through the agency of local or national associations of their instructors. Such an association, embracing college, and professional school, and academy professors and teachers, already exists in New York State. Similar ones might be formed in other sections, or for the nation at large.

(To be continued.)

Franklin Institute.

Proceedings of the Stated Monthly Meeting, January, 16th, 1869.

The meeting was called to order with the President, Mr. J. Vaughan Merrick, in the Chair.

The minutes of the last meeting were read and approved.

The Actuary submitted the minutes of the Board of Managers, and reported the donations received at their stated meeting held January 13th, inst.: from the Zoological Society, the Royal Astronomical Society, the Royal Institution, and the Society of Arts, London, and the Association for the Prevention of Steam Boiler

Explosions, Manchester, England; and l'Ecole des Mines, Paris, France.

The various Standing Committees reported their minutes, and the Special Committee on Revision of the Patent Laws reported progress.

The annual report of the Board of Managers was read by the President, and its publication was directed, by motion.

ANNUAL REPORT FOR 1868 OF THE BOARD OF MANAGERS OF THE FRANKLIN INSTITUTE.

In accordance with the By-Laws, the Board presents the following report concerning the affairs of the Institute and its operations during the past year.

The number of members and of registered stockholders is as follows:

Number as per last report			1636
Members elected in 1868	113		
Registered Stockholders added	24		
Total added	——	137	
Members deceased in 1868	22		
" resigned "	36		
Total	——	58	
Nett additions		——	79
Present number			1715

It will thus be seen that the usual annual growth in membership has been maintained.

The condition of the finances, as exhibited in the Treasurer's report, which is herewith submitted, is as follows:

Balance in treasury, January 1st, 1868	$ 1953 54
Receipts during the year	12,804 02
Total	14,757 56
Payments during the year	12,334 34
Balance in treasury, January 1st, 1869	$2423 22

Showing a surplus of receipts over expenditures of nearly $500,

During the year no increase has been made in the funded debt, which remains as per last report, 5 per cent. loan, $11,600.

To meet which there is invested in United States Bonds, bearing interest *in gold*, at 6 per cent., $7000.

Two very successful lectures have been given by the Resident Secretary, at the Academy of Music, illustrated by novel and attractive experiments. To one of these, occurring during the session in this city of the National Board of Trade, an invitation was extended to, and accepted by that body.

Other lectures have been delivered in the Lecture room of the Institute by an efficient corps of Professors, which, by the beauty and novelty of the illustrations, have, besides affording instruction to large audiences, exercised a valuable influence in adding to our memberships. It is proper to remark in this connection, that these lectures have made more apparent the limited facilities afforded by the present Hall for popular instruction in science and the arts. The movement inaugurated in the year 1867, having for its object the appropriation of the "Penn Squares," in this city, to various Societies (of which the Institute was one), for the purpose of erecting thereupon buildings or halls, and to which reference was made in the last annual report, failed to obtain the approval of the Legislature at its last session. It is hoped that during the present session successful efforts may be made to revive and pass this measure.

It is certain, that until new accommodations, in this or some equally suitable locality, be provided, the Franklin Institute cannot accomplish its full measure of utility in promoting the cause of science and the industrial arts.

The Exhibition of American Manufactures, intended to be held during the past year, and for which the City authorities granted the temporary occupancy of one of the Penn Squares, was reluctantly given up, for reasons which appeared to the Board conclusive.

No building of adequate dimensions can be had; and the expense of erecting suitable buildings, for temporary use, on the site above alluded to would have been so great, that the Board did not deem it advisable to incur the risk, especially in view of the then impending Presidential election.

It appears to be desirable that an Exhibition should be held, so soon as accommodations can be obtained for the purpose.

During the past year, the *Journal of the Institute* has been greatly improved, not only by the use of numerous illustrations, but in the nature of its contents, which have been for the most part original.

Its circulation has also so far increased that the cost of publication, now so much higher than formerly, has been nearly covered by the receipts, without any increase in the subscription price.

As its real value, under its present editorial management, becomes more generally known, its publication will prove, as it should, a source of revenue to the Institute.

All of which is respectfully submitted.

By order of the Board.

J. VAUGHAN MERRICK, *President.*

The judges of the election of officers for the ensuing year reported the following gentlemen as duly elected:

President,
J. Vaughan Merrick.

Vice-President,
B. H. Moore.

Treasurer,
Frederick Fraley.

Secretary,
Prof. Henry Morton.

Auditor, Samuel Mason.

Board of Managers,

(For 3 years.)

Charles S. Close,
John Birkbeck,
Washington Jones,
J. S. Whitney,
J. M. Wilson,
James Dougherty,
Robert Briggs,
Pliny E. Chase,

(For 2 years.)

William Helm.

(For 1 year.)

Caleb S. Hallowell,
John H. Towne.

The paper announced for the evening, on the Neva Bridge, at St. Petersburg, by Mr. Joseph Harrison, was then read.

(The report of this paper is not yet ready, but will appear in an early issue.)

The regular monthly report of the Resident Secretary, on Novelties in Science and the Mechanic Arts, was then read, after which the meeting, on motion, adjourned.

HENRY MORTON, *Secretary.*

A COMPARISON *of some of the Meteorological Phenomena of* JANUARY, 1869, *with those of* JANUARY, 1868, *and of the same month for* EIGHTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11¼′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	January, 1869.	January, 1868.	January, for 18 years.
Thermometer—Highest—degree	58·00°	46·00°	65·00°
" date	9th.	2d.	29th, '64.
Warmest day—mean	51·67	42·00	58·33
" " date	9th.	2d & 4th.	15th, '63.
Lowest—degree	17·00	11·00	—9·00
" date	26th.	10th.	8th, '66.
Coldest day—mean	25·67	18·67	—1·00
" " date	26th.	13th.	9th, '56.
Mean daily oscillation	12·13	11·79	11·89
" " range	5·85	5·80	6·41
Means at 7 A. M.	33·77	27·84	27·85
" 2 P. M.	40·27	33·23	34·97
" 9 P. M.	36·16	29·87	30 98
" for the month	36·73	30·31	31·27
Barometer—Highest—inches	30·269	30·580	30·757
" date	13th.	31st.	8th, 66.
Greatest mean daily pressure	30·260	30·531	30·665
" " " date	13th.	31st.	8th, '66.
Lowest—inches	29·478	29·291	28·911
" date	11th.	1st.	23d, '53.
Least mean daily pressure	29·667	29·450	29·086
" " " date	24th & 30th.	1st.	23d, '53.
Mean daily range	0·217	0·271	0·219
Means at 7 A. M	29·975	30·010	29·960
" 2 P. M	29·946	29·986	29·922
" 9 P. M	29·979	30 028	29·952
" for the month	29·963	30·008	29·945
Force of Vapor—Greatest—inches	0·309	0·247	0·505
" date	30th.	23d.	11th, '58.
Least—inches	·055	·048	·023
" date	26th.	9th.	22d, '57.
Means at 7 A. M	·149	·129	·132
" 2 P. M	·146	·127	·145
" 9 P. M	·159	·135	·142
" for the month	·151	·130	·140
Relative Humidity—Greatest—per cent	95·0	95·0	100·0
" date	11th & 31st	21st.	Often.
Least—per cent	37·0	30·0	24·0
" date	16th & 25th	24th.	25th, '60.
Means at 7 A. M.	73·9	80·6	79 2
" 2 P. M.	57·3	64·7	66·8
" 9 P. M.	72·1	77 8	75·8
" for the month	67·8	74·4	73·9
Clouds—Number of clear days*	8·	9·	9·
" cloudy days	23·	22·	22·
Means of sky covered at 7 A. M	71·9 per ct	57·7 per ct	62 5 per ct
" " " 2 P. M	60·0	60·3	62·2
" " " 9 P. M	42·3	52·2	48·4
" " for the month	58·1	56·7	57·7
Rain and Melted Snow—inches	3·23	3·49	3·179
No. of days on which rain or snow fell	11·	13·	10·5
Prevailing Winds—Times in 1000	S 88°27′ W. 218	N 79°43′ W. 265	N 64°44′ W. 314

* Sky one-third or less covered at the hours of observation

JOURNAL

OF THE

FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE

PROMOTION OF THE MECHANIC ARTS.

Vol. LVII.] APRIL, 1869. [No. 4

Editorial.

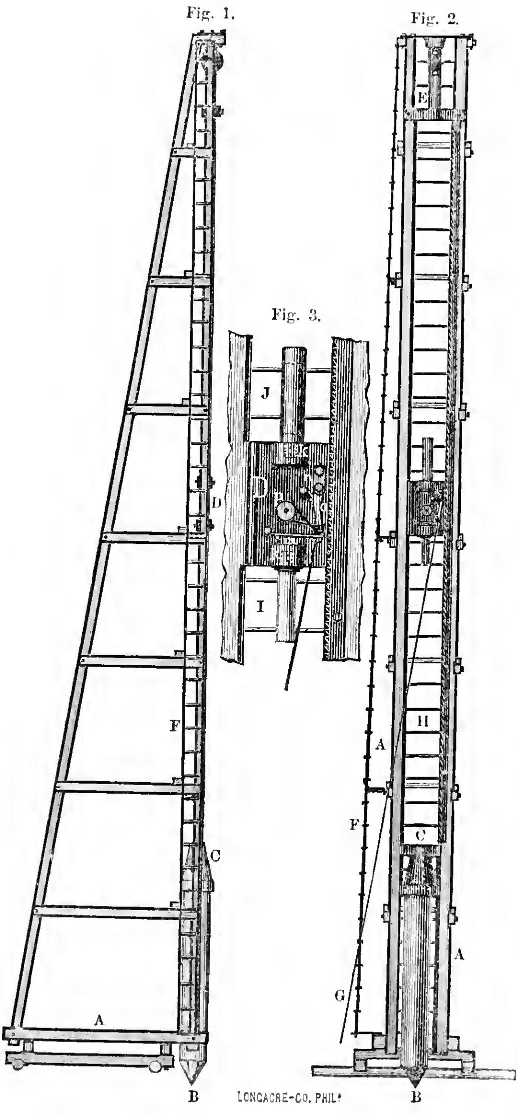
ITEMS AND NOVELTIES.

Gunpowder Hammer and Pile-Driver.—At a late meeting of the Institute, there was exhibited a gunpowder hammer, invented and constructed by Mr. Thomas Shaw, of this city. In describing the apparatus, Mr. Shaw said:—

Among the first and most useful mechanical instruments is the hammer; but the muscular effort necessary to throw the ordinary hammer, is too expensive to permit this mode of operation to be universal: hence other sources of power have been called into requisition, such as wind, water, and steam, and it is now proposed to add gunpowder to the list.

In the introduction of gunpowder for operating hammers, several advantages over all other sources of power are secured.

1st. In the employment of a concentrated fuel combined with a sufficient supply of oxidizing materials to make its combustion independent of artificial or natural draft.



2d. In the completeness with which the principle of expansion that can be brought into play, without cut-off valves or complications of machinery, and as it is only at high pressures that the value of expansion becomes prominent, we here have it to the fullest extent.

3d. In the readiness with which the machine can be started. Its going into action being simultaneous with the ignition of the fuel, and the immediate eessation in use of fuel with the stoppage of the work.

4th. In the simplicity of the maehinery. For while the gunpowder hammer is the most powerful, it has not the tenth part of the complications of other hammering appliances.

5th. In its controllability. For this most powerful of all operative forces can be regulated by a ehild.

6th. In economy; for it is less expensive than other maehines, because the fuel is employed to the best possible advantage, and

because the heat is not radiated and lost from innumerable surfaces.

The *modus operandi* of bringing about these results is as follows: A weight or hammer is suspended between vertical guides, and is provided on its under side with a plunger that fits into the bore of a cylinder held between the same guides beneath the hammer. It is intended that the cylinder should rest upon the object to be pounded, and that the hammer should be held by a pawl, which catches into a rack secured parallel with the guides. The pawl is released from the rack by a cord connected with the same, whereupon the hammer is allowed to fall. A small amount of powder is placed in the cylinder; the hammer falling, forces its plunger into the cylinder, compressing and heating the air, which explodes the powder, forcing the hammer up again, and forcing the cylinder downward with an effect fully eight times as great as from the fall of the weight alone. At the top of the guide-frame is suspended a plunger, which fits into a cylinder in the top of the hammer, thus making an air-chamber to receive the blow of the hammer in case of an over-charge of powder, that no danger may result to the machine.

I will here mention that this device is very useful as a drop-hammer, being complete within itself, and always ready for work, independent of other source of power.

Also, as a forge-hammer, the continued pressure of the blow gives this hammer evident advantage over all others for large work; for it will be observed, that the pressure is continual while the hammer is moved from a state of rest through a space equal to the length of the bore of the cylinder, which, in a large hammer, is a distance of forty inches, whereas, in other methods of hammering, the pressure is lost so soon as the momentum is overcome, in spattering the surfaces of a large mass of metal.

From this it results that large masses are never homogeneous to their centres, and the outer shell is put under a severe, crushing strain, while the inner metal is under a like tensile strain, and thus a great loss of effective resistance occurs.

Wrought metal should be kneaded and worked, as dough, to bring out its highest qualities. An hydraulic press would be the most suitable appliance, but for its slowness, which involves great loss of heat before the metal is fully manipulated.

It is here that gunpowder comes to our aid, giving the continued

pressure of the hydraulic press combined with the rapidity of the hammer, and thus renders possible the working of iron under the hammer like putty under the hands. The advantages of thus *pressing* metal are too prominent to need enumerating.

The model which was exhibited on this occasion had a ram of about three pounds weight, and a fall of eight feet. The charge employed was half a grain of white gunpowder, made of chlorate of potash, ferrocyanide of potassium, and sugar.

With a larger instrument, whose ram weighed seventy-three pounds, with a fall of twenty feet, the charge was 14 grains of the same powder.

A pile placed under this, and driven one-quarter inch at a stroke by the fall of the ram, without the use of powder, was driven two inches at each stroke, when the powder was used, and after being driven, with a square end, into hard ground, to a depth of four feet, showed no splitting or injury to its head.

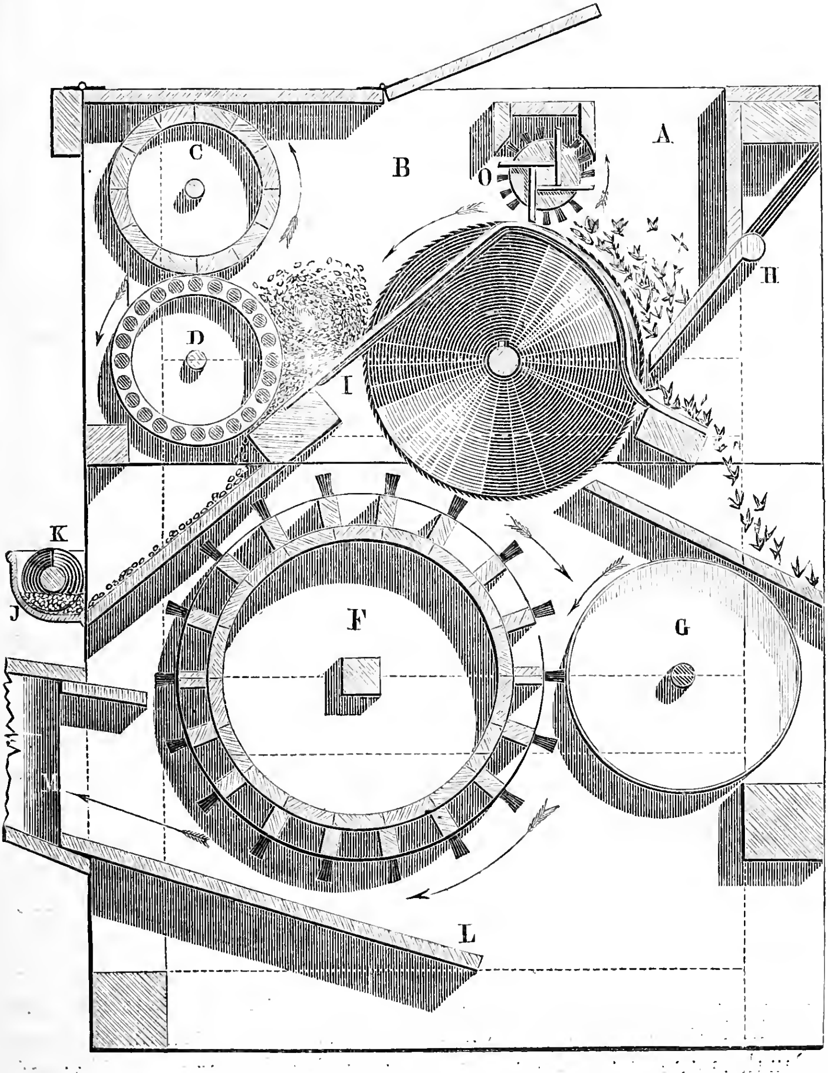
The Duplex Engine of Henry R. Worthington.—In answer to inquiries received from various quarters, as to the engine referred to by Mr. Robert Briggs, in his letter published in the *Journal*, p. 16, we would state that the engine whose performance is there ranked with that of the Cornish type, is what may be fully and historically indicated by the title, Worthington Wolfe Annular Expansive Duplex Engine. As we are referring to this article, we will take occasion to correct a typographical error which was made after revision of the proof. The numerator of the fraction expressing the maximum velocity of the plunger, should be not K, but the Greek letter π, which expresses of course, the relation of a circumference to its diameter, 3·14159.

Cotton Gin, by Richard R. Gwathmey. At the last meeting of the Institute were exhibited the above apparatus with specimens of its work.

Its structure and operation will be clear on examination of the following cut and description.

The cut represents a transverse section of the Champion Gin and Huller. The cotton is dropped into the front box, A, where the saws in operation carry it into the breast box, B, the hulls being constantly kept back by the hulling roller, O. The cotton roll which forms in the breast, B, is carried and kept intact by the two carrying rollers, D and C, the lower one of which is open or slatted, and the upper one a continuous circumference. The brush,

F, takes the cotton off the saws, and throws it on the wire gauze cylinder, G, which allows the dust, but not the cotton, to pass through it. The brush, F, gathers the cotton again from this wire



cylinder, and throws it past the mote board, L, and out of the flue, M. O shows the ribs between the saws, and H shows an inclined sliding bottom to adjust the hulling operation. The seeds pass out under the roller, D, fall into the trough, J, and are carried off by the screw, K.

The saws are so arranged as to run in a reverse direction to those of all other gins. From this new adjustment, a great advantage is claimed, namely, that of procuring the lint from the seed cotton "in the boll," without cutting or breaking the hulls or napping the lint.

It must be obvious to all who have any experience of the mode of preparing cotton for the market, that a machine which will accomplish this without any additional power required to work it, must enable the planter to cultivate and gather in much larger crops with the same number of hands, than he could possibly do by relying upon the capacities of ordinary gins, adopted only for work upon cotton previously picked from the hull.

To show the financial advantages claimed for this invention a short calculation will suffice. In the usual way of hand-picking cotton, from 150 to 200 pounds in the seed, constitute an average day's work of a hand. But when the boll and hulls are pulled or raked off, from 600 to 800 pounds a day can be carried from the field by a single hand.

Supposing the cotton to be picked from the field by the hand, in the usual way, at the rate of 200 pounds to the hand per day, the amount in 30 days would be 6000 pounds. Allowing two-thirds of this weight for the seed, the amount of cotton wool, after ginning, would be 2000 pounds of merchantable cotton, or 5 bales of 400 pounds each. This at 20 cents per pound would yield the planter $400 for thirty days picking of one hand.

But where a Gwathmey Gin is employed, which will gin the cotton thrown into the hopper, *hulls and all*, four times the amount can be gathered from the field by one hand in the same time, that is to say, 24,000 pounds in thirty days. Deduct from this gross weight, 10 per cent. for the hulls, and two-thirds of the remainder for the seed, and the yield of clean cotton wool to the planter would amount to nearly 20 bales, which at 20 cents per pound would give $1,600 for the product of one hand's picking for thirty days.

The Suez Canal.—At the last meeting of the *Institute*, the following remarks were made on this subject by Mr. Robert Briggs.

The last and best report on the progress of the work on the Suez Canal, is one just made by Mr. John Fowler, formerly President of the Institution of Civil Engineers, in London. Although very apparently made in the English interest, it will be found, upon close examination, to present a full and favorable report of the present condition of the undertaking.

Notwithstanding the elaborate reports of the French engineers, a translation of the last of which has been read by the Secretary this evening, it does not seem generally understood how far the canal has proceeded, or what the real extent of the enterprise consists in.

I will therefore state for those who are curious to know, that the total length of the canal is 99½ miles.

Its northern end is at Port Said, which is an artificial port, made for the canal, by projecting two jetties into the sea from a point on a long, narrow beach, which skirts the coast of the Mediterranean at the easterly end of the delta of the Nile. This beach, like the beaches on the shore of New Jersey, encloses an inlet or lake. The first 37 miles of the course of the canal lies in the Lake Menzaleh. The average depth of water was but four or five feet, while the depth of the finished canal is to be 26 feet. Lake Menzaleh is a stagnant, vile water, a little salter than the ordinary water of the sea, because of the rapid evaporation which occurs in this nearly rainless district. Across this lake the canal has been dredged until a clear water way of 15 or 16 feet appears to have been attained everywhere, and the full excavation is going on by means of dredging machines more rapidly than was anticipated, when the promise of completion in October next was made. After passing through Lake Menzaleh, the canal next passes higher ground between El Ferdam and Lake Timsah.

The excavation in some part of this section of 9½ miles, seems to have been about 134 feet (some statements make it only 87 feet, total cutting). This section of the canal was opened last November, and the water admitted into the depressed basin of Lake Timsah. Lake Timsah, within the historic period, has been a fresh water lake, but it has been long dried up, probably about 1,400 years.

The course of the canal, with only the requirement of dredging out the bottom, follows the lake, and the navigable terminus for vessels under 16 feet draft, is at the southerly end of the Lake Timsah, 54 miles from the Mediterranean.

Then there follows through a ridge 7½ miles of cutting, which is now approaching completion, some portions being excavated to the full depth, while others are ready for the application of the dredging machine.* When these 7½ miles, which have been considered the

* Since this was stated, the waters of the Mediterranean have been permitted to flow through this section into the Bitter Lakes.

greatest task, shall have been completed, water can be let into the Bitter Lakes. The course of the canal follows that of the valley, or depression of the Bitter Lakes.

The present surface of the ground of this valley where the course of canal is projected, is nearly all of it 26 feet below the level of the sea. It is probable that the depression of solid ground is even more, as excavations of 8, 10, 12, or in some places 30 feet, show the earth to be about three-quarters soluble salts.

Twenty-three miles of canal are thus formed ready for use. At the southerly end of the Bitter Lakes, the ridge of Chalouf, about 5 miles, with a sand-stone bottom, presents itself. The open excavation of this cut has engrossed more of the labor of the workings from on the canal, than any other section the past two years, and it is more nearly completed than any other part of the heavy work to the full depth. From this point, 11 miles to Suez, the ground is but little above the Red Sea, and on this last section the least work has been done.

There are really now only 16½ miles to be opened, or not ready for the water to flow into.

At the deep cuttings on either side of Lake Timsah, there has been trouble by the blowing in of sand, which will continue until the sand surface has arranged itself, and will probably always continue to a lesser extent. The Company are planting trees and shrubs on either bank for some distance from the canal. Mr. Fowler does not assert that the quantity of sand which will silt into the harbor of Port Said or blow into the canal will be an insurmountable detriment, but he estimates the largest quantity he can suppose as such a quantity, that "considerable expense" at the Port and one or two powerful dredges at Lake Timsah will be always needed. Again, Mr. Fowler estimates the effect of a strong current, which may arise from the evaporation of water in the Bitter Lakes. As no similar current supplies the far larger extent of water of Lake Menzaleh, it is very improbable that any quantity approaching the 250,000,000 of cubic feet of water daily will be evaporated in the Bitter Lakes. With but one or two feet depth of water, evaporation has been found to go on rapidly, but with greater masses of water, the rate of evaporation is much diminished. The Janks of India by no means corroborate any such estimate.

We may feel sure that the canal is rapidly approaching completion. Before the middle of May, the water of one, if not of both

seas, will be rushing into the depression of the Bitter Lakes, which with fair allowance for evaporation, it will take three months to fill. The great delay of the opening being in this filling of the Bitter Lakes. Whilst the estimates of the difficulties present and to come, in the engineering point of view, are, if not exaggerated, at least fully stated in Mr. Fowler's report, his estimates of the business capacity and the business opportunity of the canal are as little favorable as it is possible to assume. Were general engineering operations to be considered only from every possible point of failure, and their probable returns be estimated by the least assumable capacity, and the smallest probable use, but few engineers would be needed. England has not generally learned from her engineers how not to do everything.

The truth is, the canal has been undertaken in the face of what every good Englishman considers the interest of England, and more than that, without the aid of English engineers or English capital. Could the prices of the debentures be brought low enough, more favorable reports would appear in London.

Long Span Bridges.—We are indebted to the kindness of Prof. De Volson Wood, for the following interesting summary:—

TABLE OF BRIDGES HAVING LONG SPANS.

TRUSSED BRIDGES.

NAME OF BRIDGE.	Total length in feet.	No. of Spans.	Longest Span.	REMARKS.
Schaffhausen, Switzerland	365	2	193	Weisbach Mech. Vol. II p. 283.
Trenton, N. J.	880	5	200	Wooden arch trussed Haupt on bridge construction p. 242.
Columbia, Penn'a	5,280	29	200	Burr's—destroyed during rebel invasion, 1863. Mahan, p. 240.
Newark Dyke, Eng			$240\frac{1}{2}$	Longest span of Warren's Girder. Jour. Frank. Inst., Vol. 26, 3d Series, p. 156.
Essex, Mass	250	1	250	Mahan. Civ. Eng., p. 238.
Chepstow, Eng	606	4	306	Queen Past—Theory of Bridges—Weak.
Noget, E. Prussia	$374\frac{1}{2}$	2	321	Jour. Frank. Inst. Vol. 39, 3d Series, p. 230.
Upper Schuylkill		1	$340\frac{1}{3}$	Mahan, p. 237.
Louisville Bridge, Over Ohio River	5,201	25	370	Fink's Truss—Report of Company.
Wettingen, Germany	390	1	390	Erected in 1778. Longest span of wooden truss on record. Weisbach Mech. Vol. II. p. 83.
Dirschau, Prussia	$2,383\frac{1}{2}$	6	$397\frac{1}{4}$	Iron Lattice. Jour. Frank. Inst. Vol. 39, 3d Series, p. 230.
Kuilmburg, Holland			515	Longest span trussed bridge. Official Rep. 1866.
Derry, designed by Claus (never built		1	900	Proposed wooden structure. Weis. Mech. Vol. II., p. 84.

TUBULAR BRIDGES.

NAME OF BRIDGE.	Total length in feet.	No. of Spans.	Longest Span.	REMARKS.
Conway Eng.		1	400	Civ. Eng. Jour. 1848.
Britannia, Eng.	1,513	4	460	Tubular Bridges by Dempsey. Traite de la Construction des Ponts Metalique Pl. X.
Victoria, at Montreal, Canada	10,284	25	330	Hunt's Merch. Mag Vol. XXXI., p. 504; 24 spans are each 242 feet.

ARCHED BRIDGES.

NAME OF BRIDGE.	Total length in feet.	No. of Spans	Longest Span.	REMARKS.
Neuilly (over Seine)	more than 640	5	128	Mahan. p. 225.
Teff, South Wales		1	140	Failed by rising of the crown. Woodbury on the arch. p. 432.
London Bridge,	784	5	152	Woodbury, p. 432—for railroad purposes.
Rica, Ayr			180	Jour. Frank. Inst., Vol. 39, p. 231.
Chester or Grosnover			200	Mahan, p. 228.
Great Washington Aqueduct			200	Sc. Am. 1860. p. 86. Cast iron, by Rennie.
Southwark			250	Smile's Lives, Eng., Vol. II., p. 188.
Trizzo Adda	251	1	251	Longest stone arch on record. Treatise on Bridges. Weale, Vol I. p. 48.
St. Louis Bridge	1,509	3	515	Not yet built. The arch to be of steel Rep. by the Co. 1858
Proposed bridge over the Thames, by Telford		1	600	To be made of iron. Weisbach, Vol. II., p. 86.

SUSPENSION BRIDGES.

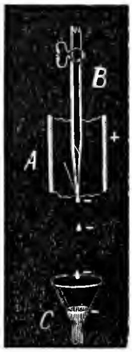
NAME OF BRIDGE.	Total length in feet.	No. of Spans.	Longest Span.	REMARKS.
Niagara Carriage Bridge		1	1,264	Sc. Am. Vol. XX., p. 218. This bridge is about a mile below Nigara Falls.
Cornwall (proposed to be built across the Hudson River, 42 miles above N. Y. City	2,499	1	1,600	Jour. Frank. Inst., Vol. LVII., p. 165.

NAME OF BRIDGE.	When Built.	Span.	REMARKS.
Douro, at Oporto	1842	558	Sup. to Weale's Bridges, p. 144.
Menai. Eng.	1825	580	Chain cable. Mahan's Civ. Eng. p. 255.
St. Johns, N. B.	1852	622	Sc Am. June 19th, 1852.
Nashville, over Cumberland	1850	656	Destroyed by Rebel Gen. Floyd, Feb. 1862. Sc. Am. Mar. 30th, 1850.
Pesth, over Danube	1849	670	Total length, 1250. Jour. Frank. Inst., Vol. XVII., 3d Series, p. 300.
Niagara Railroad Bridge	1854	822	Jour. Frank. Inst.
Fribourg, Switzerland	1834	870	Jour, Frank. Inst. Vol. XXIII., 2d Series, p. 141.
Lewiston (7 miles), below Niagara Falls	1856	1,043	Sc. Am. June 1st, 1861. Blown down, Feb. 1864.
Lexington and Danville Railroad Bridge	1856	1,220	Jour. Frank. Inst., Vol. XXXIX., 3d Series, p. 230.
East River Bridge, N. Y. City		1,600	Proposed. Jour. Frank. Inst, Vol. LXXXIV., p. 243.

Electrical Induction.—Some novel and curious applications have been made by Sir William Thompson of this fertile principle, which has already received so many important developments, among which the Holtz machine is perhaps the most remarkable.

In the first place, this distinguished student of electric science and successful inventor of delicate instruments for telegraphic signaling and measurements, describes an instrument for developing electricity by the action of drops of water. It is arranged essentially, as follows: water is made to flow in a series of drops from the extremity of the pipe, B, surrounded by the cylinder, A, which is charged feebly, say with negative electricity. Each drop as it is about to fall from the pipe, and projects beyond the lower edge of A, acquires a negative charge by induction, which it carries to the funnel, C, and which may, from it, be conveyed to, and stored in, a Leyden jar connected therewith.

Fig. 1.



Following out the principle here indicated, (which has also been applied to the development of electric charge by the action of a lamp flame,) Sir W. Thompson has arranged another machine intended especially for transmitting signals on the Atlantic cable, though capable of many other useful applications.

A wheel of vulcanite, with a large number of pieces of metal attached to its rim, is kept rapidly rotating round a fixed axis. These pieces of metal called carriers, are very lightly touched at opposite ends of a diameter, by two fixed tangent springs. One of these springs is connected with the earth (called the earth spring), and the other (the receiver spring), is connected with an insulated piece of metal called the *receiver*, which is analogous to the prime conductor of an ordinary electrical machine.

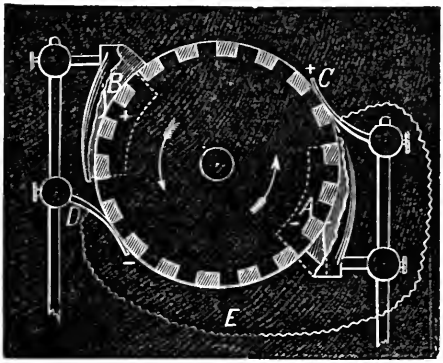
The point where the earth spring comes in contact with the carriers, is exposed to the influence of an electrified body (generally a piece of metal, insulated), called the *inductor*. When this is negatively electrified, each carrier comes away from contact with the earth spring, charged with positive electricity, which it gives up, through the receiver spring to the receiver. The receiver and inductor are each hollowed out and are properly placed to surround as nearly as may be, the points of contact of the corresponding springs.

The inductor must be kept constantly electrified. This is effected

by an adjunct, called the replenisher. This consists of an insulating wheel, with inlaid metallic blocks, revolving, as in the former case, within two inductors, A and B, and having also two conductor springs, C and D, in contact with it at the points where in its rotary motion the blocks of metal come out of the inductors.

The inductors, by means of springs, also communicate with the metal pieces while these are inside of them. Suppose, now, one inductor, A, to be charged with negative electricity, while the other, B, is neutral. The first can communicate no charge to the parts of the wheel within it, because they *are within*, for statical charge is confined to *outer surfaces*. As, however, the blocks pass beyond the edge of the inductor, A, they encounter the adjacent conductor, C, which is charged by induction from the *outside* of the inductor, so as to be positive, and to render the other conductor spring, D, which is connected with it by a wire, E, negative. The blocks thus pass away from this first conductor, A, with a positive charge, which they carry round and yield to the other inductor, B, which is thus made positive, and by its action on its conductor spring, D, increases the charge of the other one, C, and causes the blocks passing away from itself to carry negative charge to the first inductor, A. These inductors being connected with those of the other machine, keep them charged.

Fig. 2.



When the machine is started, bright flashes and sparks begin to fly about in different parts. This machine was very small, intended to be run by the clockwork of an ordinary Morse receiving apparatus. No doubt, much greater effects could be obtained with an enlarged apparatus.

Spectrum Lines—a new method of illustrating them. At a late meeting of the American Institute, Professor E. C. Pickering, of the Massachusetts Institute of Technology, employed the following method to illustrate his paper on Spectrum Analysis. A sheet of black lace, one and a half feet broad, and three feet high,

was suspended as a screen, and upon it was thrown a continuous spectrum from a magnesium light, arranged in the manner first developed for the electric light, by Prof. Cooke, of Cambridge, except that only one bisulphide of carbon prism was used. The spectrum covered the entire lace screen, the curvature of its lines being corrected by an opposite curvature of the opening through which the light passed. Upon this black lace, were attached a number of white paper strips, so arranged as to occupy the places of the bright lines in seven different spectra, such as those of sodium, potassium, rubidium, cœsium, &c. These being illuminated by the variegated light of the continuous and broad spectrum, received and reflected each the color corresponding to its position, and therefore (their adjustment being accurate), that which actually belonged to the band which it represented. The light falling between these paper bands was not of course reflected, and the appearance therefore was in each case (as in an actual spectrum), of bright colored lines on a dark ground. When, however, as in the case of potassium, [the nebula of Orion, &c., a faint continuous spectrum is in fact combined with that of the bright lines, this also was represented by attaching in the required places, bands of white lace, which reflected enough of the colored light to produce a hazy spectrum, admirably imitating that of the substances or bodies in question.

The Spectrum of the Great Nebula in Orion, as seen with the Cambridge refractor, shows a faint continuous spectrum in addition to the three bright lines recognized by Miller and Huggins, and also another faint line in the vicinity of G, though its position has not yet been accurately determined. These facts, for which we are indebted to the observations of Prof. Winlock, of Cambridge, seem to indicate that this nebula is not composed of purely gaseous masses, but is made up of a number of gas-enveloped stars (like τ of the Northern Crown, during its eruption in 1864,) or, possibly, of a congeries of stars and gaseous spheres.

Should the new bright line near G prove to be coincident with the most refrangible line of Hydrogen (which coincides with a marked solar line a little less refrangible than G,) we shall have additional evidence of the presence, in this interesting object, of the omnipresent element—Hydrogen.

Lunar Volcanoes.—We observed, lately, in other journals, a notice that a lunar volcano in activity had been discovered by Prof. Winlock, of Cambridge. Through the kindness of Prof. Mayer, of

Bethlehem, we can now give the exact facts of the case. Observations with the great Cambridge refractor were made upon the moon by Prof. Winlock, with the view of bringing to the question of lunar volcanic activity a new class of evidence.

If any portion of the moon's surface were luminous through heat, it would yield a continuous spectrum without the Fraunhofer lines which distinguish light reflected from the sun. On observing the brightly-illuminated edge of Aristarchus, a spectrum was obtained which was marked by a bright streak running through its middle across the lines, whose width it seemed to diminish. It was, therefore, on the spur of the moment, suggested that this indicated the presence of self-luminous or highly-heated matter.

On further examination, it was, however, found that exactly the same effect was observed when the instrument was turned upon any other similarly brilliant point.

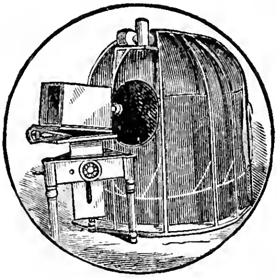
Petroleum in Place of Bisulphide of Carbon to Vulcanize India Rubber.—The use of the last-named of these substances in combination with chloride of sulphur has been common for many years in the process of vulcanizing, but is open to many objections, the most serious of which are, 1st. The great loss experienced from the extreme volatility of this liquid, and 2d. Its injurious effect upon the health of those inhaling its vapor, not to mention the disgusting nature of the odor it evolves when this is first perceived.

It has been stated, lately, that this last property is due to an impurity which may be removed; but the olfactory nerves very soon lose perception at all events, if its presence is continued. In view of all these considerations, however, it is evident that such a substitutions as is named above will be of value, if, as is stated, it proves practically successful.

Testing Machine.—A writer to "*Engineering*," remarks that in some tests which he witnessed with the Kirkaldy machine, a trammel used on the bar itself, showed a far less amount of extension under various strains, than was indicated by elaborate machinery and index attached to the instrument for this purpose. This is an important point to have in view in connection with such tests.

Photography at Night.—A few evenings since, some experiments were made at the salesrooms of Messrs. Wilson & Hood, with the apparatus devised by Mr. G. K. Proctor, of Salem, Mass., for the purpose of taking likenesses after dark.

The accompanying cut represents the arrangement, which consists of a light frame, from which is supported an oval inclosure of paper-cloth, of a highly reflecting character. The sitter being placed within, and a back-ground screen adjusted, a little magnesium is burned by means of a lamp, or otherwise, at the top, and its light diffused by reflection from the interior of the paper walls, enables a good negative to be made in 15 to 20 seconds, during which time the cost for light is about 9 cents. Many excellent pictures have been thus made by the inventor, who says that often the best part of his days' work is done at night. The efficiency of the arrangement consists in the *nearness* of the *light-colored walls*, to the sitter, by which much light is thrown upon him in various directions and in a diffused manner, thus avoiding the gloom and painful local intensity common to pictures by magnesium light in ordinary rooms, where the scattered light is lost before it can be reflected upon the sitter from the *distant* walls.



Trial of a Steam Road Roller.—A public trial was made at Rochester, on Friday, January 15, in the presence of a number of officers of Royal Engineers and others, of a powerful steam road roller recently manufactured by Messrs. Aveling & Porter, at their works, at Strood, and intended to be sent out to New York, to be used on the roads of the Central Park in that city. The engine was purposely tested under the most disadvantageous circumstances, with a view of fully developing its power, for which purpose it was made to ascend Star-hill, the steepest incline in the city, which has a rise of one in twelve. The entire surface of the roadway had previously been thickly covered with stones of the ordinary kinds used on macadamized roads. The steam roller commenced its work soon after 10 o'clock, and notwithstanding the increased difficulties it had to surmount, by 4 o'clock in the afternoon, it had made repeated ascents and descents of the hill, the entire surface of which was rolled completely smooth and fit for the passage over it of the lightest vehicles. In addition to other orders they have received, Messrs. Aveling & Porter, the patentees, have received directions from the Government to manufacture three of the rollers for India.

Editorial Correspondence.

ACID PROOF CEMENT.

Philadelphia, March 22d, 1869.

Mr. Editor:—Thinking that the following suggestion may be of use to, or improved upon by some of the readers of the *Journal of the Franklin Institute*, I offer it with these ideas for what it may be worth. It frequently happens that the chemist requires some means for protecting cork from the destructive action of vapors such as arise from boiling nitric acid. I think that the plan here mentioned, may possibly meet this requirement. Finding it necessary to connect a glass tube with a wide-mouthed flask, and in so doing to use a cork which was exposed to the action of the fumes from boiling nitric acid for several hours, I found the best preservative to consist of a coating of silicate of soda and powdered glass. The cork having been bored to suit the size of the tube, was soaked for two or three hours in a solution of silicate of soda, consisting of one part of commercial concentrated solution, to three parts of water. The tube was next inserted, and when dry, the cork was covered with a paste made by mixing the condensed solution of the silicate with powdered glass in such proportion as to form a mass of about the same consistence as that of putty. This is spread on the under surface, and then washed with a solution of chloride of calcium. It soon hardens, so I think it advisable to make the connection with the flask whilst the paste is in a plastic state, and to allow it to become solid before applying heat to the vessel containing the acid.

Corks protected in this manner, were but slightly acted upon, though remaining over the boiling nitric acid more than four hours, and over hot acid for ten. In some instances, when not entirely covered, the vapor softened the cork beneath the silicate to the depth of about a quarter of an inch, but the cement proved sufficiently strong to form a compact diaphragm, enabling the tube to be removed from the flask without danger of the fluid contained being contaminated. I would suggest also the application of this cement as a luting for chemical apparatus for general use, as I find that it remains unaffected even when immersed in strong nitric, sulphur, or muriatic acids. The immersion in these liquids was made whilst the plaster was still soft, with the only perceptible effect of hardening the same immediately. Yours, &c.,

Rob't. F. Fairthorne.

Civil and Mechanical Engineering.

BELTING FACTS AND FIGURES NO. II.

By J. H. Cooper.

(Continued from page 164.)

The Telo-dynamic System.

"For the transmission of power or 'work' to moderate distances, we have had, for ages, two main methods employed—the horizontal revolving shaft and the strap pulley—supposing, as is almost always the case, that what we need in the *form* of the transmitted work is *rotatory* motion; but these are only capable of transmitting work to extremely short distances, unless with the most serious losses or waste of power by the absorption of work of the motor in torsion and friction of the shaft, and in friction, rigidity and slip, &c., of the strap."

"For a mere dead pull, such as the alternate strokes needed to work a pump, work is, and has long been, transmitted to very great distances; as by the long lines of 'draw rods' used in mining regions for transmitting the power of a water wheel by means of a crank on its main axis, *pulling* during half its revolution to raise a heavy weight or 'balance bob' at the remote end of the line of bars; but a system of draw rods cannot economically be employed in producing rotatory motion at great distances.

"In recent days, the idea so clearly seen by Bramah has been realized upon a large scale by Armstrong and others, in the transmission of power by water pressure, or, as it has been called, by 'hydraulic connection;' and Armstrong has even perfected apparatus by which water pressure thus transmitted, through it might be, miles of pipe, may be converted into rotatory motion."

"Papin saw the possibility of transmitting power through pneumatic conductors, though if the accounts handed down be reliable, he did not succeed in realizing his notions. There can be no question, however, that power may be transmitted, either by exhaustion or by the condensation of air, for very great distances through tubes, and may be converted into rotatory motion at the remote extremity, or anywhere by the way. * * * * *

"In both these cases, like that of the hydraulic connection, however, unless the tubes bear a very large proportion in area to the demand for the current, whether of entering or of issuing air that transmits the power, the loss by the friction becomes very serious. The capital to be sunk in pipes, therefore, is large in relation to the power got, and both this expenditure and the waste of power increase directly with the distance of transmission."

"Meanwhile, however, there exists in actual use, and upon a large scale, another and a simpler apparatus for the transmission of power, in large amount and to very great distances, which has attracted, we may say, no attention as yet in this country. We refer to the system originated by Mr. C. F. Hirn, of Mulhausen, which has been called that of 'telo-dynamic transmission,' and some drawings indicative of which were shown as long ago as the Exhibition of 1862."

"Crudely stated, this method appears to the superficial observer to consist in nothing more than in transmitting the rotatory motion of one large grooved pulley, kept revolving by the motor, to another such pulley at a greater or less distance by the intervention of an endless wire or steel band, or wire rope, passing over both pulleys; and to the uninstructed observer the whole affair seems little else than the old 'belt and pulley,' a mere elongation of that commonplace 'wrapping connector.' The hidden principle involved, however, is something entirely different.

"If we suppose a band of round iron of one inch in diameter to be capable of sustaining a steady pull, without sensible alteration, of ten tons, and that the bar be pulled with this force endways, so that a point between the motor and the resistance moves at the rate of one foot per second, then it is obvious that the bar itself will be transmitting 'work' at the rate of ten foot-tons per second. A bar of half its diameter, or one-fourth its section, can only be strained to 2·5 tons, and at the same rate of 'end-on' motion, can only transmit 2·5 foot-tons of work per second; and so also of a bar one-fifth of an inch diameter, or one-twenty-fifth of the area of the one-inch bar, it can only transmit ·04 ton, and, at the rate of one foot per second, ·04 foot-tons of work. But suppose that the half-inch bar moves end-on at the rate of four feet per second, and that the one-fifth inch diameter wire moves at the rate of twenty-five feet per second, then, as work is made up of pressure, times velocity, all three bars, much as they differ in section and in absolute strain upon each, will transmit the same number of foot-tons per second:

i. e., shall all be capable at the resisting end of delivering forth equal quantities of motive power in equal times. If, therefore, we increase the velocity of motion of the wrapping connector, which is intended to transmit a given amount of motive work in a given time, we may reduce its section, because we have reduced the strain upon it, and hence its total weight in the inverse ratio of the increased velocity. We may, in fact, to put an extreme illustration, reduce the one-inch round bar to an iron wire, as fine as a human hair, and yet (theoretically) get out of it at the resisting end our ten foot-tons per second."

"Now, this is just the principle which distinguishes Mr. Hirn's method from any common belt and pair of pulleys, and which he has shown can be carried into practical use with great advantage.

"At the motor, be it steam engine or water wheel, he places a tolerably large cylindrical-grooved rimmed iron pulley, revolving in a vertical or horizontal plane, to which he communicates rotation at a determinate and considerable speed. Round this he passes a thin wire, or a thin wire rope (which latter in practice he prefers), and this is led away to almost any reasonable distance (the limit is measurable by *miles*), where it is passed over another similar pulley, and returns back as an endless cord to the pulley whence it started. If the distance be more than a few hundred feet, or the intervening surface differ in level, &c., both limbs of the cord are supported and guided at intervals by guide pulleys, as few as possible, leaving the cords in the intervals between these to sag down into such catenary as the strains upon it and its own surplus strength may determine and admit. The periphery of the driving pulley may have an angular velocity as great as possible; the only limit, in fact, is that the speed shall not be likely to destroy the pulley by centrifugal force. The speeds that have been actually employed in the examples to which we are about to refer, vary from ten to thirty yards per second, at the circumference of the pulley. The pulleys themselves have been made of cast iron and of steel, and they have but one peculiarity of construction, and that is a highly important one. At the bottom of the acute V-shaped groove, going round the circumference, a little trough is formed, dove-tailed, in section, which is filled with a ring of softened gutta-percha let into it, and united at the returned ends. Against this, as the bottom of the V groove, the wire rope of transmission alone bears. It forms a seat for itself, and does not touch the sides of the V groove or other metallic

parts of the pulley. The same arrangement is adopted for the receiving pulley for the power at the remote end, and for all guide or supporting pulleys that may be necessary.

"The wire ropes are thus found to hold perfectly, and not to wear sensibly, for long periods.

"Previously to devising this form of pulley, Mr. Hirn had much difficulty from the wear of the wire ropes, and in other ways, and had tried, in vain, wood, copper, and other linings for the pulley grooves.

"Now, assigning a peripheral velocity to the motor pulley of thirty yards per second, it is obvious, upon the principles we have already stated, that for each horse-power that we require to transmit, we must visit a strain upon the material of the cord, moving at that rate, of $\frac{33000}{90} =$, say 366 pounds; and taking the breaking strain for average iron wire at 67,000 pounds per circular inch, and the safe strain at about one-half that, or say 33,000 pounds per circular inch, then $\frac{33000}{366} = 90$ nearly, or *a wire of one-ninetieth of a circular inch in area will transmit a horse-power per second.*

"The wire must have surplus strength, however, also, for the *loss* of power absorbed by the sources of loss in this method of transmission. These are: 1, the resistance of the air to the rotation of all the pulleys; 2, the rigidity of the wire rope in circumflexure of the two main pulleys, and through the change of angular direction at either side of supporting or guide pulleys; 3, the resistance of the air, by friction, to the passage of the wire cord itself through it; 4, the friction of the axles of all the pulleys.

"Where the distances of transmission are moderate, *i. e.*, within a few hundred yards, the actual result of experiments upon the large scale, as stated by Mr. Hirn, show that all these together amount to about $2\frac{1}{2}$ per cent. of the power transmitted. Where supporting and guide pulleys are required, there is to be added to this $2\frac{1}{2}$ per cent., which represents a *constant* resistance due to the motor and transmitting pulleys and rope merely, an *additional* resistance which *varies* with the distance, and has been found, with the usual amount of supporting or guiding pulleys needed for long distances, to amount to about 504 foot-pounds for each 1100 yards in length of the double cord. Thus we see that for short distances the transmitting wire need not be larger than one-eighty-fifth of a circular inch in area per horse-power; and for even such an extreme distance as upwards of *twelve miles*, we need only add one-

third to its total area, on account of all resistances due to uselessly consumed power.

"Enough has been said to show how attenuated may be the transmitting cord, and how light it may be.

"With two pulleys, each of twelve feet diameter, making 100 revolutions per minute, and with a wire cord of two-fifths of an inch in diameter, Mr. Hirn has found that 120 horse-power can be transmitted to a distance of 150 yards, with only a waste of power or useless effect of 2½ horse-power.

"The first attempt at practical application of this method was made as long ago as 1850, at Logelbach, near Colmar, at the ancient calico print works of M. M. Haussmann, established in 1772, but shut up from 1841. It was proposed to make the great concern into a weaving factory for cotton, but the immense scattered mass of buildings seemed to forbid the possibility of utilizing them, and yet placing the motive power at any one point. Shafting, as a matter of cost and of waste by distance, was out of the question.

"In this emergency Mr. Hirn first tried this method of force transmission, with a rivetted steel ribbon or band to each building from the engine-house. The band first tried was about two inches wide by one-twenty-fifth of an inch thick, and on wood-faced drums. The success of the *principle* was complete, but much remained to be discovered before the round wire cord and gutta-percha pulley solved all difficulty, and brought the principle to be a practical reality.

"Since the establishment of M. M. Haussmann's weaving mills on this plan, in 1854, M. Schlumberger has transmitted the power of a turbine at Staffelfelden about 90 yards; in 1857, at Copenhagen, 45 horse-power was transmitted to saw mills at more than 1000 yards distance from the motor; in 1858, at Cornimont, Vosges, 50 horse-power was transmitted to a distance of 1258 yards; in 1859, at Oberursel, near Frankfort-on-the-Main, 100 horse-power was thus carried 1076 yards; and at Emmendingen, Brisgau, 60 horse-power works a spinning mill at 1312 yards from the motor; while in 1861, Count d'Espremesnil, at Fontaine le Sonet, Department de l'Eure, transmitted a very large power to saw mills through 1100 yards, and thence a part to a further distance of 546, or to a total of 1646 yards, to drive other machinery.

"*Four hundred and upwards* of practical applications have been made of the method already; which has become one of the estab-

lished and recognized mechanical appliances all through the south-east of France and adjacent Germany, and more particulary in Alsace, the great seat of the French cotton manufacture, and of innumerable connected industries.

"Most of the machinery has been constructed by Messrs. Stein & Co., of Mulhausen, who have acquired great experience in its constructive details and management. And, carrying the principle out to its legitimate end and developement, a company has been formed since 1862 at Bale, in Switzerland, under the title of "Compagnie d'Utilization des forces du Rhin Superieur," whose object it is to establish water power to an almost limitless extent at the great fall of the Rhine at Schaffhausen, and thence to transmit it off to great distances and in various directions, and let it off for manufacturing uses. Mr. Hirn has shown that power may be practically and profitably transmitted thus for distances of several miles. To take an extreme example, he proves that 120 horse-power may be transmitted 22,000 yards, or about 12½ miles, and that the loss by uselessly expended power in the transmission even to that extreme distance, shall leave, at the very least, 90 horse-power available at the remote end. On the other hand, he shows that if 200 horse-power be transmitted by ordinary horizontal shafting, fifty per cent. of the motor will become uselessly absorbed within a distance of 1650 yards, and that to obtain 100 horse-power at the remote end of a horizontal shaft 12½ miles long, we must apply at the motor end a power of 788,400 horse-power: in other words, that while the limit of shaft transmission practically does not exceed two or three hundred yards, that of telo-dynamic transmission need not, in practice, stop at five miles, or even more. Power may thus be transmitted economically and surely, in any direction, over hills and valleys, across rivers, into the depths of coal pits or mines."—***Practical** Mech. Jour., March*, 1867, *p.* 358.

(To be continued.)

The experiments which have been made over the telegraph lines between Harvard College and San Francisco, show that the traveling time required by electricity is as follows from Boston: To Buffalo and back, 0·10 seconds. To Chicago and back, 0·20 seconds. To Omaha and back, 0·33 seconds. To Salt Lake and back, 0·54 seconds. To Virginia City and back, 0·70 seconds. To San Francisco and back, 0·74 seconds.

THE DREDGING MACHINE.

By D. S. Howard, C. E.

No dredging machine is equally well calculated for all kinds of dredging, under various circumstances. Some underwater excavations are required to be continued through dry land, and the deposits made on the banks; at other places the height of the banks requires the excavated material to be put into lighters. As the lighters in such cases cannot be placed alongside at all times, the deposits must be made over the stern. Other work is situated in water sufficient on all sides to float the machine and lighters, which requires another modification of the machinery, in order that it may be economically adapted to the situation.

There are variations of the material also, in which no invariable machinery will work equally well. Wherefore I have generally heretofore constructed dredging machines expressly for particular works, without any view to the execution of any other work. But I have frequently found circumstances to vary the conditions of the same work so much, that two or more machines, very different in construction, were required to complete it with economy.

From these considerations, after long experience in constructing and working dredging machines of various descriptions, under a great variety of circumstances, I have prepared the following plans of a machine, which has been found capable of being adapted to a very large range of circumstances and situations.

Such a construction is more valuable, since the improvements made in the materials, the manufacture and the construction of the machinery render it so much more durable, that its usefulness is extended beyond the immediate purposes for which it was originally constructed.

Fig. 1 is a side view of a dredging machine calculated to deposit in lighters, over the stern or at the sides, as occasion may require. It is also calculated to work with any shaped bucket best adapted to the material to be excavated.

Fig. 2 is an end view of the same, showing the facilities for lateral deposit. The same letters refer to like parts in each.

A is the driving chain-wheel, geared to the engine by the wheels and shafting, shown in Figs. 1 and 2.

B designates the buckets attached to the chains shown in Figs. 1 and 2.

C represents the centre cylinders, with the spiral scrapers and hooks, for loosening and conveying the material to be excavated, from the centre, each way, to the buckets.

D, in Fig. 2, represent short cylinders on the ends of the cylinder shaft, E, Fig. 1, with spiral scrapers for conveying the material to be excavated, from the outside to the buckets.

The short cylinder on the port side of the dredge, Fig. 1, is left off, to show the lower chain-wheels and the lower attachments to the ways.

F is one of the lower chain-wheels, attached to the cylinders and driven by the chains, in the same manner as the chains are driven by the driving chain-wheel, A, by cams fitting into alternate links of the chain.

G is one of the flange-wheels seen in Fig. 1, over which the chain passes before descending to the lower chain-wheels, F.

H is one of the movable frames, which suspends the flange-wheels, G, on rollers running upon ascending ways, provided with powerful purchase-wheels, terminating in a pinion working in a descending rack, shown in Fig. 1.

This purchase is worked by a cog-wheel on the flange-wheel shaft (not shown in the drawing), working in a wallower-wheel, J, when thrown into gear, for the purpose of raising the buckets from the bottom when required, by forcing the flange-wheels, G, up the inclined ways. When the machine is not in motion, the wallower-wheel may be thrown out of gear, and worked by hand, with a long double crank, K, Fig. 2.

By the motion of the buckets in the direction of the arrows, the excavated material is brought up, and if it is to be deposited over the stern, it is dumped into the movable spout, L, Fig. 1, which is attached to the axis of the flange-wheel, G, by the extension of its sides, and moves freely within the stationary spout, M, allowing the flange-wheel, G, to be drawn up, when necessary, with the movable frame, H, without changing its proper position, for receiving the contents of the buckets as they pass over the flange-wheel, G.

N, Fig. 1, is the counter balance, situated about 25 feet aft of the hull, attached to it by a truss frame and hog chains, for the purpose of balancing the weight of machinery necessarily placed ahead of the hull, to enable the dredge to clear its own way. It is also

Pl. I.

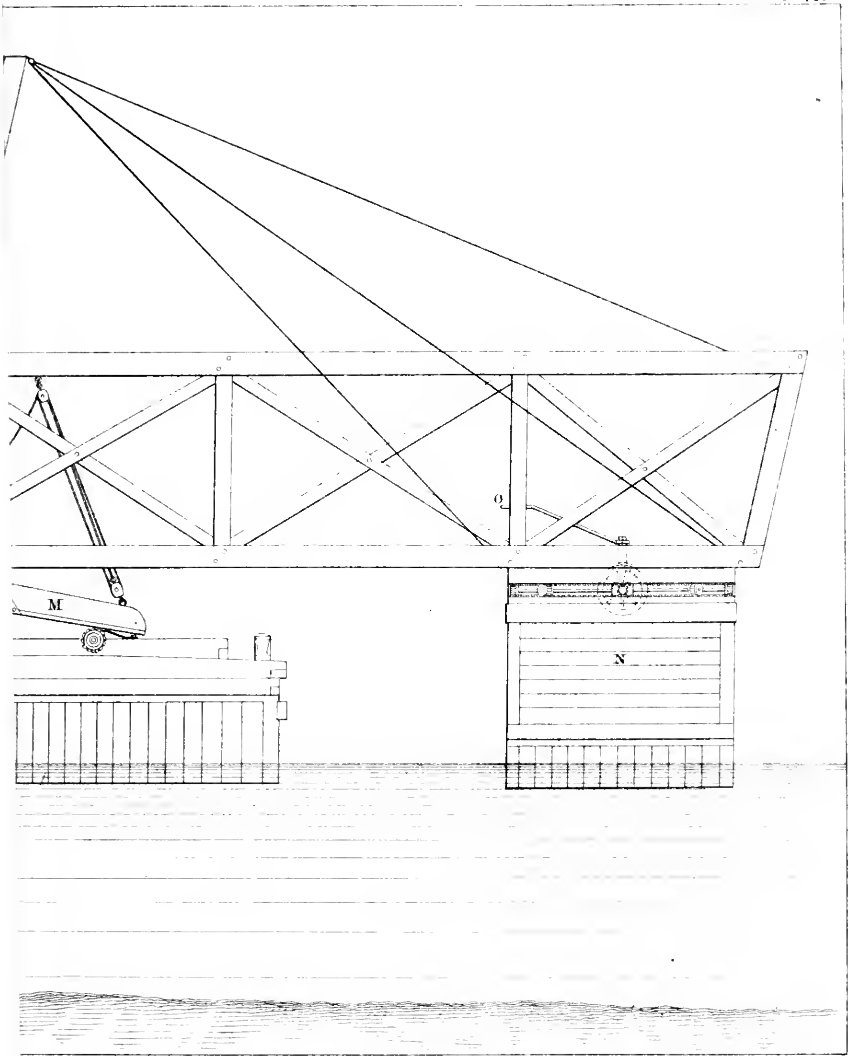
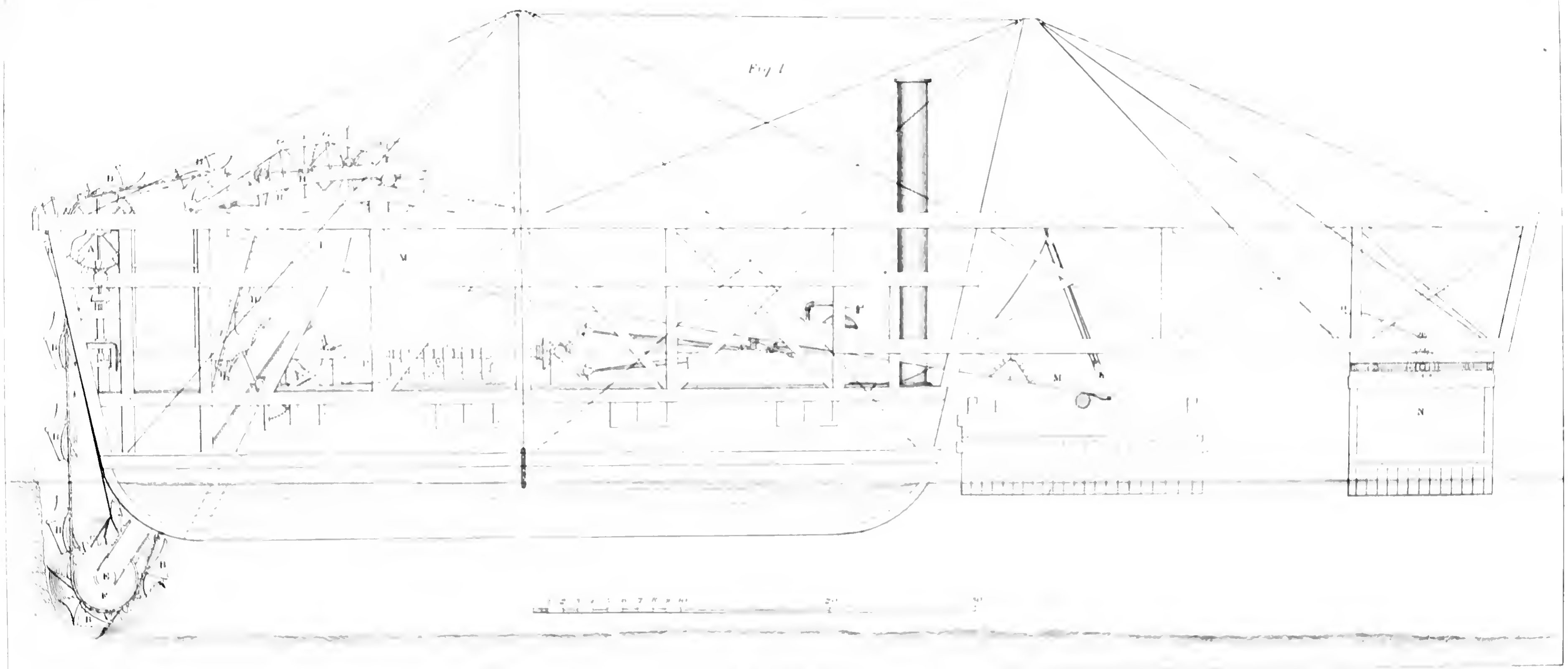


Fig. 1



used for transferring the lighters, by attaching the empty one to the outside of the counter balance, and the loaded one to the inside; then by the lever, O, which works the geared rollers between the counter balance and the truss frame, the two lighters are changed about, the empty one inside, under the spout, M, ready to be filled, and the loaded one outside, ready to be transferred to the dumping place. It is also used as a water tank for the engine, which may be filled in the morning, before the water has been disturbed by the dredging, sufficiently to afford clean water during the day.

B, Fig. 3, is a perspective view of a bucket used in this arrangement for depositing over the stern. It is provided with a loose bottom, which drops with the load about two inches, rendering the discharge perfectly certain when at work in the worst kind of material.

When the situation of the work is such as to require the deposit to be made on the bank, or into the lighters alongside, a bucket like B, Fig. 4, is put on the chain, in place of B, Fig. 3, which dumps into the lateral spouts, situated between the driving chain-wheel, A, and the flange-wheel, G, by the tripping of the latch which lets fall the whole under side of the bucket, hinged to the bolt that fastens the bucket to the chain. This insures the discharge of the most difficult material.

The short receiving spouts under the buckets, and the one between them, are hung on pivots in the centre, so that either end may be elevated, and the contents of both sets of buckets discharged on either side, or both sides, as may be desired.

Whenever the extent of the work requiring a lateral deposit is sufficient, the hull of the dredge may be built long enough, and lean enough aft, to balance the machinery forward, without the counter balance.

When the work is situated in water deep enough in front of the dredge to float the loaded lighters, the buckets like B, Fig. 5, which dump through the bottom, are brought into requisition, the latch to these may be tripped anywhere on their perpendicular way up, and discharged into a short vibrating spout, which conveys the material directly into the lighter, placed much nearer than it can be in any other arrangement. Thus saving power in proportion to the height of discharge.

Fig. 6 is a gang of hooks, sometimes put upon the chains between the buckets, when working in hard, coarse material, like cobble-

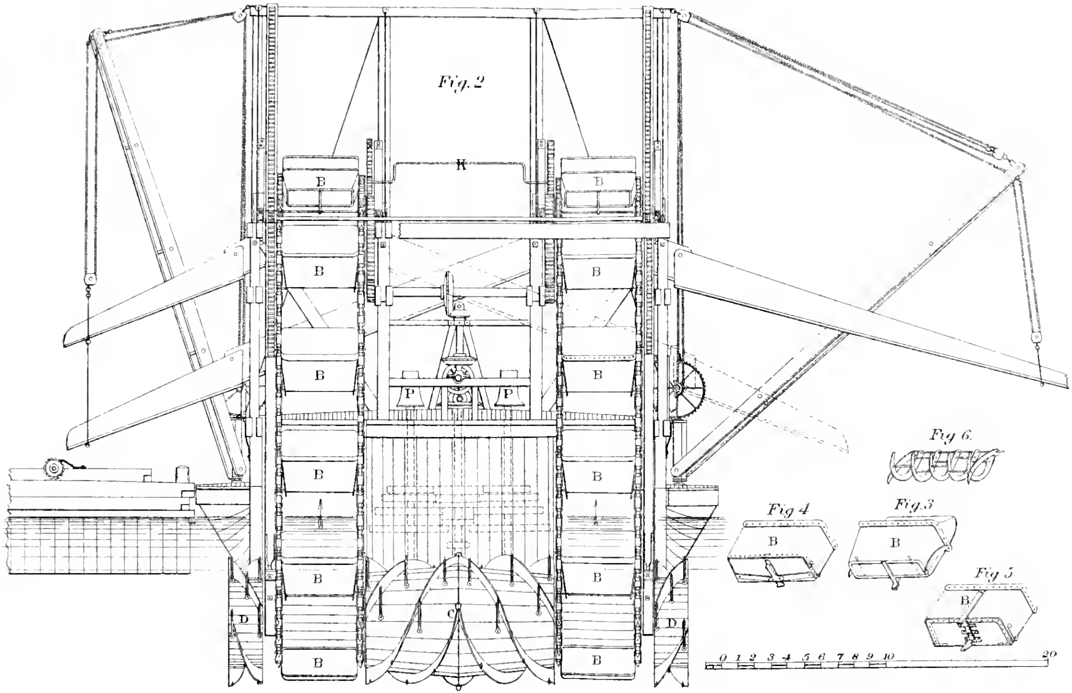
stone, shale, or hard-pan. The chain is so constructed that any shaped bucket, or any other device for loosening the material, that might be found in practice to be preferable, may be put upon it. All the articles here represented have been fully tested, and found very useful in their places. No sacrifice of power, or of economy in working, has been required to enable us to use all these appliances on the same machine; on the contrary, the perpendicular position of the working part of the chains, and their passing around three drums instead of two, are great improvements under all circumstances.

The perpendicular application of power secures a great economy of friction, in the wear of rollers, ways, &c., especially where no lubricating material can be used, nor the wearing parts secured from the destructive action of sand and water, which must always be present in dredging.

The third drum, which constitutes the flange-wheels, G, enables us to raise the buckets from the bottom, in the manner above mentioned, without changing the perpendicular position of the working part of the chain. It also furnishes the best possible position for the discharge of buckets like B, Fig. 3, at all times when that kind of bucket is in use. Also, that of B, Fig. 4, which dumps between the two upper drums, requiring nearly an horizontal position to discharge.

The bucket B, Fig. 5, is equally well accommodated. This one may be dumped anywhere on the perpendicular part of the chain, by raising or lowering the vibrating spout which trips the latch. This is the most economical of all the different buckets, where circumstance are favorable for its use, as it dumps with perfect freedom all kinds of material, and is discharged at a much less elevation, on account of its better relative position with respect to the lighters.

In working these machines, a pully is anchored at a convenient distance ahead, with a feed line passing from one of the feeding capstans, P, through the pully and back to the other capstan, either one of which, or both, may be worked by the adjustable machinery to any required motion, by a change in the series of clutches at Q, or a series of wheels below deck; while the spiral cylinders below water, with the buckets, clear the way to the full width of the dredge, and to the depth required, bringing up the material and depositing it in lighters, or on the banks, or at any distance in any direction horizontally, not exceeding one in twelve of the altitude



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overcome, by supporting a spout, lined with sheet iron, of the length required, on a movable support, that it may coincide with the feed motion of the dredge.

The distance from the dredge at which the deposit is required, determines the length and inclination of the spouts. Something, however, depends upon the nature of the material excavated. If it contain clay, or vegetable matter sufficient to prevent the water from draining out too soon, it will run on a descent of one inch to the foot, without more water than the buckets bring up with it; but if the material be pure sand or gravel, a pump will be required to assist in the discharge, without more inclination of the spout.

These machines can be constructed with any dimensions, to suit the magnitude of the work, provided the proper proportion of the parts be preserved for strength, motion, power, and durability. These considerations are very important for the success of any machinery, more particularly such as is supported on a floating foundation.

As yet, I have never been called upon to build a dredging machine that would excavate more than three thousand cubic yards in a day of ten hours, but one may be built, with economy, to raise double that amount, rather than build two for the same work, as only the same number of men are required on the dredge, all the additional help being that needed to dispose of the excavated material.

The Central American Transit Company, with William H. Webb as President, and J. E. Body, Vice-President, built one of these machines, on a small scale, in 1867, for the purpose of improving the San Juan river, in Nicaragua, and the harbor at its mouth. This was capable of raising three thousand cubic yards per day, and was worked with great satisfaction, until, by some misunderstanding between the Company and the Government of Nicaragua, the work was suspended.

Another machine, with some of the above improvements, was built in 1857, for the Corpus Christi Ship Channel Company, in Texas, with which the Channel was finished to sixty-four feet in width, without the use of a lighter, except to support the long spout in which was run off the excavated material, on an inclination of one inch to a foot.

Lyons Falls, N. Y., Dec., 1868.

EXTRACTS FROM AN ENGINEER'S NOTE BOOK.

By W. M. Henderson, Hydraulic Engineer.

(Continued from page 168).

Combustion as applied to Steam Boilers.

In a properly constructed steam-boiler, an average of ten pounds of coal will be burned per square foot of grate per hour with natural draught, and as one pound of good coal will evaporate about eight pounds of water, eighty pounds or about ten gallons of water will be evaporated for each square foot of grate surface per hour. When blast is employed, these quantities may be doubled. The furnace room should be from 1·5 to 2·5 cubic feet per square foot of grate, according to the consumption of coal. The boiler room should be about one cubic foot for each square foot of heating surface. The area of heating surface should not be less than one square foot per pound of coal consumed per hour, or, where it can be obtained, as 18 to 1. of the grate surface. Area over bridge, through flues and chimney, two square inches per pound of coal consumed; or, taking twelve pounds of coal as being the maximum that can be burnt upon a square foot of grate, a calorimeter of twenty-four square inches per square foot of grate surface is recommended for natural draught. Ash-pit entrance for air, one-half area of grate. Area of orifices for admission of air above grate should be about four square inches for each square foot of grate, more or less, according to the gas-generative qualities of the coal and extent of combustion. In regard to the disposition of the heating surface, it should be as concentrated as possible, as experiments have shown that the first foot of the length of tubes in horizontal boilers, next the furnace, was equal in evaporative value to the furnace itself; that the next four feet were not equal to the first foot, and that the fifth foot possessed very feeble generative properties. It is therefore considered there is no gain in having tubes in such boilers over six feet in length, and that the effective length of flues in horizontal boilers is arrived at within very moderate limits, probably not exceeding the maximum of twenty feet. The reason of this is quite obvious, as a boiler may be of such length that the generative value of the heated gases may be entirely expended before they have reached the extreme end, in

which event, it is plain, a positive loss is entailed; for the water which has been heated at the furnace end, and the steam which has been raised there, must be deprived of a portion of that heat again, in order to raise equally the temperature of the water at the cold end.

For many years, within the experience of the writer, it was the practice to construct steam-boilers from 40 to 60 feet in length, with a view to use up the whole of the heat resulting from combustion of the fuel. Some of those very boilers, after being cut in two—the one-half only retained—gave such improved results as to have at last completely exploded that plausible theory. Quite an important feature in the generation of steam is to be found in the manner of conducting and applying the heated gases to a steam boiler. They should be led at once through the body of the water, *i. e.*, through flues or tubes, and if a return can be made, with a view to economy, to make such return under the shell, and if practicable, with a still further view to economy, over the shell of the boiler. No doubt much of the merit pertaining to the Cornish type is due to this manner of construction. Of the double-flue description, the Butterley, or Fish-mouth, or, as it is better known in this country by the more modern name of the Corliss boiler, is far preferable to the common flue boiler, where the heat passes backwards, first under the boilers and lastly through the flues, passing the most effective heating surfaces at the time it has no real generative value. Another matter of eminent importance to this subject, and one which has not received that degree of attention its worth demands, is that of supplying to the uninflamed gases of the furnace the necessary amount of oxyen to insure perfect combustion, which otherwise would escape unconsumed into the atmosphere. This is the only method of obtaining from the fuel the maximum of heat it is capable of yielding. By experiment it has been shown that the saving effected in coal, by a judicious attention to this particular, has been from 12½ to 33 per cent., accompanied with a perfect freedom from smoke, as all smoke consists of a portion of the carbon of the fuel passing off unconsumed.

It is believed this discovery, like many others of great merit, was the result of mere accident, not being brought about from any hope of economy, but the desire to suppress the intolerable nuisance of smoke, emitted from the furnaces in the manufacturing districts of England, from the combustion of bituminous coal. In the com-

bustion of anthracites, as largely employed in this country, no great objection has been found to the comparatively colorless smoke emitted, and so it has been allowed to pass unheeded; but as anthracites contain more carbon than bituminous coals (as high as 90 per cent.), even more air is required for their combustion than the latter description; and unless this air is supplied, large quantities of carbonic oxide will be constantly discharged into the atmosphere, carrying off a large per centage of the carbon of the fuel, without contributing in a proper manner to the generation of steam. As regards the form of the furnace, many curious ideas exist. It has been contended that the crown sheet should be low enough, so that the flame may impinge with *force* upon it. The value of position of the heating surfaces have been settled for us, by some authority, long ago, and has been religiously copied in all our works of modern compilers, who inform us that only horizontal surfaces should be considered, and calculated, as direct heating surface; that vertical sheets must only be taken at half value, and that the upper half of the circumference of flues alone are effective. Now, caloric being a body which radiates in all directions, the transmission of heat through the different parts of a steam-boiler cannot vary to such an extent as the above calculations lead us to believe. It is true the heated gases have a tendency to rise to the highest points, but where the calorimeter is properly calculated to carry away the net products of combustion, it cannot ascend in this manner, and must tend to diffuse itself over every avenue, in order to effect an exit. A large firebox or capacious furnace is necessary to produce a thorough admixture of the gases of combustion, which undoubtedly should take place in that part, where the necessary heat is present to insure this result. The concentrated action of the heated currents upon the fire box must be more injurious with a low-crown sheet than a high one; in either case it would be bad enough, were it not for the great difference which exists between the temperature of the box and the water within the boiler, which seldom or never exceeds 400°, whereas that of the furnace may proably reach as high as from 1500° to 2000°.

The absorbent material should be as thin as possible, therefore of the highest standard of quality, in order to save time in the transmission of heat, and to effect a rapid evaporation of the water contained within the boiler. As regards the effective evaporating results, or transmission of heat through metals, this depends upon the

three following properties: 1st, the resistance of the surface next the fire to absorption of the heat; 2d, the resistance of the internal particles of the metal to the conduction of heat; and 3d, the resistance of the surface next the water in giving off the heat. The relative evaporating power of wrought iron, brass and copper are, respectively: 100, 125, and 156.

(To be continued.)

LOCATION OF INDEPENDENT STEAM FIRE AND BILGE PUMPS IN STEAMERS.

By Chas. H. Haswell, C. E.

Read before the Institution of Naval Architects, London. March 19. 1869.

Of all the instruments and appendages connected with a marine steam engine and the provident fitting of a steamer, the Independent Steam Pump, or Donkey, as it is termed, stands pre-eminent in importance; both on account of its general utility and its being under many very probable and oft occurring circumstances indisputable to the safety of a vessel.

The application of this instrument extends from the ordinary operations of a bilge pump and a boiler feed pump to those of a fire and a hold discharge pump.

When, therefore, the extended and in many cases, the indispensable operations of this instrument are duly considered, it would seem to be imperative that it should be located where it can be most readily reached, and where it can be operated for the longest period without being submerged by water, enveloped in smoke, or cut off by fire.

It occurs, however, that as a rule of British and European practice, that it is located in the hold of steamers, immediately upon the lower engine-room floor; this practice, taken in connection with the insufficient capacity of the pump for the general purposes of fire and leaks, would seem to have arisen from the functions of the pump, as viewed by the constructors of the engines, being restricted solely to the operation of feeding and pumping out boilers.

The cases in which the location of the pump in the manner referred to, are objectionable, are as follows:

1st. A leak occurring in the furnace or water-bottom of a boiler,

and the hot vapor arising therefrom precluding access to the pump and the setting of it in operation; whereby the water escaping from the boiler, not being replaced by the operation of this pump, would soon expose the tubes or crown-plates of the furnace to be burned, and the boiler thereby rendered unsafe for operation.

2d. A leak in the hull of a steamer suddenly occurring from a collision with ice, another vessel, a pier or sunken rock, or the disruption of the propeller-shaft stuffing box in a propeller, whereby the influx of water would be fully equal to the combined capacities of the pump and engines to discharge. The distance between the pump and the floor of the vessel or the level of the water in the hold, would be so little, that any arrest of the pump from continuous operation for adjustment or repair, would involve its submersion before it could be set in reversed operation.

3d. A fire occurring on the main deck of a steamer, whereby the smoke would be drawn into the engine and fire-rooms by the draught of the furnaces of the boilers, and this pump rendered inutile by the impracticability of reaching it to set it in operation.

4th. A vessel grounded or stranded upon a sand or soft bottom and leaking: her pump from the ingress of sand and mud into her hold, would require frequent clearance, the delay consequent upon which would cause the pump to be submerged before it could be freed and set in operation.

The only defence that ever has been advanced for this violation of regard to the safety of a vessel, in locating this pump where in many cases, as is here shown. it would necessarily be inoperative, is, that in the event of the hold being flooded with water, that the fires in the furnaces of a boiler would consequently be drowned, and that the steam wherewith to operate this pump being obtained from the boilers, that its functions would cease with the drowning of the fires.

Admitting this position to be strictly tenable, it does not meet the conditions of this pump being arrested in its operations by sand, mud, smoke, or the escape of steam. The position advanced, however, is not one of general application, as in a majority of cases, and especially in this country, there is an independent boiler connected with this pump, which when located upon the main or span deck, and acting independently of the engine boiler, its functions would not be affected by the influx of water into the hold of a vessel.

The common plea, that a pump located in the hold of a steamer, below the water line, will operate more effectively than if located upon the main deck, is only advanced by some engine drivers, whose conceptions of a steamer are restricted to the operations of the engines and their dependencies, or by some owners of steamers, who are disinclined to incur the cost of a removal of the pump to a proper location.

In this connection, the capacity of this pump proportionate to the dimensions of the vessel, is worthy of consideration, and as a further rule of practice, the capacity of this pump in British and European steamers is much inferior to that in use in this country.

The general security of lines in British vessels, under the stringent orders and requirements of Lloyd's rules, has not generally opened the subject of the propriety of using a steam pump for other purposes than those referred to, as confined to the operation of the boilers of a vessel; for in many cases, this pump cannot be used to draw water from the bilge or hold of a vessel, and has not any fire hose connections beyond the immediate precincts of the engine-room.

The capacity of this pump, proportionate to the vessel in this country, may be judged of from the following cases:—

A British steamer, now in this port, built upon the Clyde as late as 1867, and belonging to a leading company in the extent and character of its trade, presents the following cases:—

$\frac{l - \frac{3b}{5} \times b \frac{b}{2}}{95} = 3{,}230$ tons, has but one independent pump, having a water cylinder of one gallon, or a discharging capacity of 12,000 gallons per hour, or 3·7 gallons per ton per hour.

The new steamers of the Pacific Mail Steamship Company, built in this city in 1867, present the following case:—

$\frac{l - \frac{3b}{5} \times b \frac{b}{2}}{95} = 4{,}200$ tons have four independent pumps, having water cylinders of 11·4 gallons capacity, or a discharging capacity of 136,500 gallons per hour, or 32·5 gallons per ton per hour.

In further support of the position assumed, I submit that there has occurred very many cases where steamers have foundered, burned, or been wrecked in consequence of the location of their independent steam pumps in their holds. Three cases, and import-

ant ones, so far as the loss of lives and property are concerned, can but be familiar to all.

The Arctic, foundered at sea, 1854, had two independent pumps of a combined discharging capacity of 31,000 gallons per hour, and an attached boiler, all of which were located in her hold. When her bottom was perforated by collision with the sharp prow of an iron steamer, the influx of water was superior to the capacity to discharge it, and for the following causes:—

1st. Her independent boiler could not be put in operation until steam was raised in it.

2d. Her floor being filled in solid, her bilge injections could not operate effectually, until the water was fully two feet in depth, in consequence of the roll of the vessel.

3d. At the time sufficient water had flowed into the hold for the bilge injections to operate with their full capacity, the additional depression of the vessel, occasioned by the influx of water, had reduced the number of revolutions of the engines, and consequently their capacity to discharge the inflowing water; added to which, the water at that height washed the water bottoms and ash pits of the boilers, and reduced their capacity to furnish steam to operate the engine.

Thus, with a constant flow of water, the capacity to discharge it was being rapidly lost.

It occurred however, that there elapsed a period of four hours after the collision before the vessel sunk, and as a computation made by me of her entire weight as a mass, less the actual displacement of every particle of her and her cargo, gave but a difference of 1,000 (one thousand) tons, it appears that the discharge of this weight of water, or 65,570 gallons per hour, would have kept this vessel free of water, and enabled her so far as that collision was concerned, to have reached a port of safety.

Now, as the capacity of her bilge injections at even the reduced number of revolutions of ten would have been equal to 80,000 gallons water per hour, it is manifest that the loss of this vessel at this time, is directly attributable to the location of her independent pumps and their attached boiler.

The Austria, burned at sea, in 1858, in consequence of the smoke from a fire upon her main deck pervading the engine-room, suffocating the watch, and as a consequence precluding the operation of her pump as a fire-engine.

The Britannia, lately foundered at sea, by the flow of water in her engine-room, and the submerging of her independent pump.

The remedy I propose for this objectionable manner of the filling of a steamer, is to require this pump to be located upon the main or tonnage deck, and that it have an independent boiler attached to it, located upon the main deck, or, preferably, upon the upper deck, and that all passenger steamers be required to have the boiler ready for operation during the night, or during the prevalence of a fog.

Ejector Condenser.—We see in several of the English journals, accounts of a new piece of apparatus devised by Mr. Alexander Morton, of Glasgow, which, on the principle of the Giffard Injector, is intended to maintain the vacuum of a condensing engine by the escaping velocity of the residual steam on the exhaust side of the piston. We do not attempt to reproduce any of the drawings which have been published because they are all evidently fancy sketches, and of very bad proportion, so that a better idea can, we believe, be conveyed by a simple description. The apparatus consists of a series of concentric nozzles, arranged somewhat as in the simpler forms of the Giffard Injector, where no provision is made for adjustment. First of all comes a small central steam nozzle connected directly with the boiler, this is surrounded by a water jet, and is employed to start the flow where a head of water is not attainable, then follow two steam nozzles connected with the exhaust parts for either end of the cylinder.

Lastly, a trumpet-shaped outlet tube like the induction nozzle of the Giffard. The water being once started the condensation of the steam in the nozzles will give sufficient velocity to the outflow to keep up a good vacuum, even removing large quantities of air from the cylinder.

Prof. Rankine has prepared a thorough discussion of the theory, and tabulated the result of many experiments made with this instrument. There is one feature of doubt appearing in these experiments, however. The amount of heat in the escaping water is largely in excess of that due to the steam doing work in the cylinder. If this represents live steam, used direct from the boiler through the first nozzle to effect the action of the apparatus, the economy of the instrument disappears.

Mechanics, Physics, and Chemistry.

ANALYSIS OF HOT CAST PORCELAIN WITH SOME REMARKS ON ITS COMPOSITION.

By Chas. P. Williams.

(Late Prof. Analytical Chemistry, Polytechnic College.)

"Hot Cast Porcelain" is the trade-name of a peculiar, tough, milk-white, translucent glass, not unlike French porcelain in appearance, now extensively manufactured in Philadelphia and Pittsburg. It is intermediate in character with the glasses produced by the addition of phosphate of lime to the ordinary glass materials, on the one hand, and the white enamels, such as are produced by oxide of tin, on the other, having more of the milkiness than the former, and less opacity than the latter.

The raw materials consumed in its production are sand, oxide of zinc and cryolite (native double fluoride of aluminium and sodium) which are melted together in the ordinary clay pots of the glass maker. After complete fluxing, and when in a state of quiescent fusion, the material or "metal" is capable of all the manipulations of the ordinary glasses.

The following is the composition of an average sample of this glass, taken when the "metal" was in its best working condition: Silicic acid, 63·84; alumina, 7·86; sesquioxide iron, 1·50; protoxide manganese, 1·12; oxide zinc, 6·99; lime, 1·86; magnesia, 0·25; soda, 10·51; fluorine, 8·05; [less oxygen (corresponding to the fluorine) 3·39] = 98·59. The above is the mean of five analyses by myself; two analyses by Mr. Wm. Main, Jr., in my laboratory give a mean agreeing very closely with that given above.

The method pursued in the above analyses consisted in fusing the compound with carbonate of soda, extracting with water, and, after precipitating the silica and alumina in the aqueous solution by means of carbonate ammonia, separating the fluorine as fluoride calcium, mixed with carbonate of lime, by the addition of chloride of calcium. The mixed salts were afterwards treated with acetic acid, and the separated fluoride of calcium, dried, burned and weighed. The remainder of the analyses was conducted as is usual in the analysis of silicates, the alumina and oxide of iron being precipitated as basic acetates and separated, after weighing, by means of

tartaric acid and sulphide ammonium; the oxide of manganese precipitate as binoxide in the acetic acid solution, by means of bromine;* the zinc precipitated as a sulphide by sulphydric acid in presence of acetic acid, and weighed either as a sulphide (by ignition with sulphur in a current of hydrogen gas) or as an oxide; the lime thrown down as an oxalate and weighed as caustic lime, and the magnesia precipitated by phosphate soda. Separate portions were taken for the alkali determinations and treated with fluohydric acid in the usual way.

Admitting the fluorine to exist in the glass in the condition of the silico-fluoride of sodium ($Na\,F\,SiF_2$), the following will express the composition of the compound:

	Per cent.				
Silicic acid	59·59	Oxygen		31·78	
Alumina	7·86	"	3·66	} 4·11—R_2O_3	
Sesquioxide iron	1·50	"	·45		
Protoxide mang.	1·12	"	·25	} 3·85 — RO	
Oxide zinc	6·99	"	1·38		
Lime	1·86	"	·53		
Magnesia	·25	"	·10		
Soda	6·18	"	1·59		
Sodium	3·25	} = 13·28 per cent. Silico fluoride sodium.			
Silicon	1·98				
Fluorine	8·05				
	98·66				

Which agrees very closely with the formula—

$$2\left(R_2\,O_3\,3\,SiO_2 + 3\,(R\,O\,3\,SiO_2)\right) + NaF\,SiF_2.$$

The protoxide of manganese results from the addition of binoxide to the materials for the purpose of destroying the color from the iron in the sand, etc. The total amount of soda (10·51 per cent.) corresponds to 23·84 per cent. of cryolite in the original mixture, which would contain 12·92 of fluorine, showing, therefore, that 4·87, or about 39 per cent. of the whole amount of fluorine is eliminated, by the melting operation in the form of fluoride silicon (SiF_2), requiring for its formation 3·85 per cent. additional of silica.

These figures would give as the approximate composition of one

* I have found the substitution of bromine for chlorine in the precipitation and separation of manganese to give very satisfactory results. It is particularly adapted for small amounts of manganese as in the ordinary run of iron ores, especially if the iron and alumina have been separated as basic acetates.

hundred parts of the original mixture when introduced into the melting pots, 67·19 parts of silica, 23·84 of cryolite and 8·97 of oxide of zinc. This, I believe, does not materially differ from the formula used in the practical operations at the factories in Kensington and West Philadelphia.

The rationale of the process appears to consist in the formation of a silico-fluoride of sodium from a part of the fluorine and sodium of the cryolite, the remainder of the fluorine uniting with silicon for the production of fluoride of silicon in which form it escapes from the pot. The remaining silica, uniting with oxide of zinc, the soda and alumina, resulting in the formation of a compound silicate not differing essentially (except in the substitution of oxide of zinc for lime or some other base commonly employed in glass manufacture) from some of the varieties of glass. Throughout this glass the fused silico-fluoride is distributed, acting in the same manner, though differing in the degree of its effect, as the phosphate of lime, so long employed for the production of milky glasses, for it has been shown by Brezelius that the alkaline-fluorides, in presence of silica, fuse at a bright red heat, without the evolution of fluoride of silicon, and pass, on cooling, into porcelain-like masses,* in the same manner as does the bone phosphate of lime.†

Besides the beautiful white glass produced in this manner, an additional benefit is derived by the employment of cryolite, from the fact that this mineral furnishes to the manufacturer of glass a comparatively cheap source of soda, dispensing with all the manipulations necessary to the production of soda ash either from cryolite or from common salt.

I am informed by W. J. Cheyney, Esq., of Philadelphia, the patentee of this new article of glass, that an article corresponding essentially in properties with "Hot Cast Porcelain," can be produced by the substitution of fluor-spar (fluoride of calcium) for cryolite. The milkiness in this case may probably be due to the formation of a corresponding silico-fluoride of calcium. I have verified this statement by melting together a mixture of feldspar, sand, fluor-spar and soda-ash, obtaining a product much resembling the genuine article. I have not yet been able to complete an analysis of this product.

* Gmelin's Chemistry, vol. iii., page 374, English edition.

† Ib. Page 192. According to Saussuer, the fusion takes place at 378° Wedgewood.

The "Hot Cast Porcelain" is capable of being colored by the metallic oxides usually employed in the manufacture of colored glasses, but the beautiful white of the body of the material heightens greatly the effect of these coloring matters. Both the white and the colored materials now find extensive employment in the manufacture of druggists' and perfumers' wares, pedestals for oil lamps, lamp shades, table ware and flooring tile. The patterns for this last are chaste, and the effect exceedingly pleasing. I am informed that in point of durability they are superior to the imported encaustic tiles.

It may be well to state that the action on the pots of the materials used in the manufacture of this glass, is no greater than obtains in the production of the other glasses. The pots used in Philadelphia are made from a mixture of German and Missouri clays, tempered with the baked material from old, broken pots.

Philadelphia, March 13th, 1869.

FORMATION OF AN ARTIFICIAL SPECTRUM WITH A FRAUNHOFER LINE.

By A. Wallner.

If, by means of a Holtz machine, at a short distance, the rapid discharges of a Leyden jar of about 30 square centimes are passed into an ordinary Geissler tube, and if the tube is placed before the slit of a spectroscope, the spectrum of the gas which fills the tube is first seen. If the length of the discharge is increased a little, the sodium line immediately appears as in the case of induction currents, by heating the capillary part of the tube placed before the slit. With a proper length of spark the brilliancy of the sodium line far exceeds that of the spectrum of the gas. By further increasing the distance of the discharge, the calcium line is produced with such intensity that it cannot be seen to greater advantage by any way hitherto known. Finally, if the length of the spark is again augmented, the phenomenon changes, the light in the tube assumes a dazzling splendor, this luminous line forms a continuous brilliant spectrum in which the spectroscope reveals a completely black line instead of the sodium line; this, therefore, is a Fraunhofer line. If the tube is attentively examined after this experi-

ment, one can connect the phenomenon with the explanation which M. Kirchhoff has given of the spectrum rays. The inner surface of the capillary tube is thickly corroded in consequence of the particles of glass taken away by each discharge, so that by prolonging the experiment, the glass may be completely roughened. These particles immediately raised to incandescence by the discharge, give the sodium line; but soon the tube is filled with sodium vapors, which then absorb the light proceeding from the solid incandescent particles; these form a kind of solid incandescent nucleus surrounded by an atmosphere of vapor.—*Ann. de Pogg.*, cxxxv.

ON THE RELATION OF HYDROGEN TO PALLADIUM.

BY THOMAS GRAHAM, F. R. S.

(Master of the Mint.)

It has often been mentioned on chemical grounds that hydrogen gas is the vapor of a highly volatile metal. The idea forces itself upon the mind that palladium, with its occluded hydrogen is simply an alloy of this volatile metal in which the volatility of the one element is restrained by its union with the other, and which owes its metallic aspect equally to both constituents. How far such a view is borne out by the properties of the compound substance in question will appear by the following examination of the properties of what, assuming its metallic character, would fairly be named hydrogenium.

Density.—The density of palladium when charged with 800 or 900 times its volume of hydrogen gas is perceptibly lowered, but the change cannot be measured accurately by the ordinary method of immersion in water, owing to a continuous evolution of minute hydrogen bubbles which appears to be determined by contact with the liquid. However, the linear dimensions of the charged palladium are altered so considerably that the difference admits of easy measurement, and furnishes the required density by calculation. Palladium in the form of wire is readily charged with hydrogen by evolving that gas upon the surface of the metal in a galvanometer containing dilute sulphuric acid, as usual.* The length of the wire before and after a discharge is found by stretching it on both occa-

* *Proceedings of the Royal Society*, p. 422, 1868.

sions by the same moderate weight, such as will not produce permanent distension, over the surface of a flat graduated measure. The measure was graduated to hundredths of an inch, and by means of a vernier, the divisions could be read to thousandths. The distance between to fine cross lines marked upon the surface of the wire near each of its extremities was observed.

Experiment 1.—The wire had been drawn from welded palladium, and was hard and elastic. The diameter of the wire was 0·462 millimetre; its specific gravity was 12·38, as determined with care. The wire was twisted into a loop at each end, and the mark made near each loop. The loops were varnished so as to limit absorption of gas by the wire to the measured length between the two marks. To straighten the wire, one loop was fixed, and the other connected with a string passing over a pulley and loaded with 1·5 kilogramme, a weight sufficient to straighten the wire without occasioning any undue strain. The wire was charged with hydrogen by making it the negative electrode of a small Bunsen battery consisting of two cells, each of half a litre in capacity. The positive electrode was a thick platinum wire placed side by side with the palladium wire, and extending the whole length of the latter, within a tall jar filled with dilute sulphuric acid. The palladium wire had, in consequence, hydrogen carried to its surface for a period of one and a half hour. A longer exposure was found not to add sensibly to the charge of hydrogen acquired by the wire. The wire was again measured and the increase in length noted. Finally the wire, being dried with a cloth, was divided at the marks, and the charged portion heated in a long narrow glass tube kept vacuous by a Sprengel aspirator. The whole occluded hydrogen was thus collected and measured; its volume is reduced by calculation to Bar. 760 m.m., and Therm. 0° C.

The original length of the palladium wire exposed was 609·144 m. m. (23·982 inches), and its weight 1·6832 grm. The wire received a charge of hydrogen amounting to 936 times its volume, measuring 128 c.c., and therefore weighing 0·01147 grm. When the gas was ultimately expelled, the loss as ascertained by direct weighing was 0·01164 grm. The charged wire measured 618·923 m. m., showing an increase in length of 9·779 m. m. (0·385 inch). The increase in linear dimensions is from 100 to 101·605; and in cubic capacity, assuming the expansion to be equal in all directions, from 100 to 103·908. Supposing the two metals united without

any change of volume, the alloy may therefore be said to be composed of—

	By volume.	
Palladium	100·	or 95·32
Hydrogenium	4·908	or 4·68
	104·908	100·

The expansion which the palladium undergoes appears enormous if viewed as a change of bulk in the metal only, due to any conceivable physical force, amounting as it does to sixteen times the dilatation of palladium when heated from 0° to 100° C. The density of the charged wire is reduced by calculation from 12·3 to 11·79. Again, as 100 is to 4·91, so the volume of the palladium, 0·1358 c.c. is to the volume of hydrogenium 0·006714 c.c. Finally, dividing the weight of hydrogenium, 0·01147 grm. by its volume in the alloy 0·006714 c.c. we find—

Density of Hydrogenium .... 1·708

The density of hydrogenium, then, appears to approach that of magnesium, 1·743, by this first experiment.

Further, the expulsion of hydrogen from the wire, however caused, is attended with an extraordinary contraction of the latter. On expelling the hydrogen by a moderate heat, the wire not only receded to its original length, but fell as much below that zero as it had previously risen above it. The palladium wire first measuring 609·144 m. m., and which increased 9·77 m. m., was ultimately reduced to 599·444 m. m., and contracted 9·7 m. m. The wire is permanently shortened. The density of the palladium did not increase, but fell slightly at the same time, namely, from 12·36 to 12·12; proving that this contraction of the wire is in length only. The result is the converse of extension by wire-drawing. The retraction of the wire is possibly due to an effect of wire-drawing in leaving the particles of metal in a state of unequal tension, a tension which is excessive in the direction of the length of the wire. The metallic particles would seem to become mobile, and to right themselves in proportion as the hydrogen escapes; and the wire contracts in length, expanding, as appears by its final density, in other directions at the same time.

A wire so charged with hydrogen, if rubbed with the powder of magnesia (to make the flame luminous), burns like a waxed thread when ignited in the flame of a lamp.

Experiment 2.—Another portion of the same palladium wire was charged with hydrogen in a similar manner. The results observed were as follows:—

Length of Palladium wire	488·976	m. m.
The same with 867·15 volumes of occluded gas	495·656	"
Linear elongation	5·68	"
Linear elongation on 100	1·3663	"
Cubic expansion on 100	4·154	"
Weight of palladium wire	1·0667	grm.
Volume of palladium wire	0·08072	c. c.
Volume of occluded hydrogen gas	75·2	"
Weight of same	0·00684	grm.
Volume of Hydrogenium	0·003601	c.c.

From these results is calculated:—

Density of hydrogenium	1·898

Experiment 3.—The palladium wire was new, and on this occasion was well annealed before being charged with hydrogen. The wire was exposed at the negative for two hours, when it had ceased to elongate.

Length of palladium wire	556·185	m. m.
Same with 888·303 volumes hydrogen	563·632	"
Linear elongation	7·467	"
Linear elongation on 100	1·324	"
Cubic expansion on 100	4·025	"
Weight of palladium wire	1·1675	grm.
Volume of palladium wire	0·0949	c. c.
Volume of occluded hydrogen gas	84·3	"
Weight of same	0·007553	grm.
Volume of hydrogenium	0 003820	c. c.

These results give by calculation:—

Density of hydrogenium	1·977

It was necessary to assume in this discussion that the two metals do not contract or expand, but remain of their proper volume on uniting. Dr. Matthiessen has shown that in the formation of alloys generally the metals retain approximately their original densities.*

In the first experiment already described, probably the maximum absorption of gas by wire, amounting to 935·67 volumes is attained. The palladium may be charged with any smaller proportion of hydrogen by shortening the time of exposure to the gas (329 volumes

* *Philosophical Transactions*, 1860, p. 177.

of hydrogen were taken up in twenty minutes), and an opportunity be gained of observing if the density of the hydrogenium remains constant, or if it varies with the proportion in which hydrogen enters the alloy. In the following statement, which includes the three experiments already reported, the essential points only are produced :—

TABLE.

Volumes of Hydrogen occluded.	Linear expansion in millimetres.		Density of Hydrogenium.
	From	To	
329	496·189	498·552	2·055
462	493·040	496·520	1·930
487	370·358	373·126	1·927
745	305·538	511·303	1·917
867	488·976	495·656	1·898
888	556·185	563·652	1·977
936	609·144	618 923	1·708

If the first and last experiments only are compared it would appear that the hydrogenium becomes sensibly denser when the proportion of it is small, ranging from 1·708 to 2·055. But the last experiment of the table is perhaps exceptional, and all the others indicate considerable uniformity of density. The mean density of hydrogenium, according to the whole experiments, excluding the last referred to, is 1·951, or nearly 2. This uniformity is in favor of the method followed for estimating the density of hydrogenium.

On charging and discharging portions of the same palladium wire repeatedly, the curious retraction was found to continue, and seemed to be interminable. The following expansions, caused by variable charges of hydrogen, were followed on expelling the hydrogen by the retractions mentioned :—

	Elongation.	Retraction.
1st experiment	9 77 m m	9·70 m. m.
2d "	5·765 "	6·20 "
3d "	2·36 "	3·14 "
4th "	3·482 "	4·95 "
		23·99

The palladium wire, which originally measured 609·134 m. m.,

has suffered by four successive discharges of hydrogen from it, a permanent contraction of 23·99 m. m.; that is, a reduction of 5·9 per cent. in its original length. The contractions will be observed to exceed in amount the preceding elongations produced by the hydrogen, particularly when the charge of the latter is less considerable. With another portion of wire the contraction was carried to 15 per cent. of its length by the effect of repeated discharges. The specific gravity of the contracted wire was 12·12, no general condensation of the metal having taken place. The wire shrinks in length only.

In the preceding experiments the hydrogen was expelled by exposing the palladium placed within a glass tube to a moderate heat short of redness, and exhausting by means of a Sprengel tube; but the gas was also withdrawn in another way—viz., by making the wire the positive electrode, and thereby evolving oxygen upon its surface. In such circumstances, a slight film of oxide of palladium is formed on the wire, but it appears not to interfere with the extraction and oxidation of the hydrogen. The wire measured—

		Difference.
Before charge......	443·25 m. m.	
With hydrogen...............................	449·90 "	+ 6·65 m. m.
After discharge................................	437·31 "	— 5·94 "

The retraction of the wire, therefore, does not require the concurrence of a high temperature. This experiment further proved that a large charge of hydrogen may be removed in a complete manner by exposure to the positive pole—for four hours in this case; for the wire in its ultimate state gave no hydrogen on being heated *in vacuo.*

That particular wire, which had been repeatedly charged with hydrogen, was once more exposed to a maximum charge, for the purpose of ascertaining whether or not its elongation under hydrogen might now be facilitated and become greater, in consequence of the previous large retraction. No such extra elongation, however, was observed on charging the retracted wire more than once; and the expansion continued to be in the usual proportion to the hydrogen absorbed. The final density of the wire was 12·18.

The wire retracted by heat is found to be altered in another way, which appears to indicate a molecular change. When the gas has been expelled by heat, the metal gradually loses much of its power to take up hydrogen. The last wire, after it had already been

operated upon six times, was again charged with hydrogen for two hours, and was found to occlude only 320 volumes of gas, and in a repetition of the experiment, 330·5 volumes. The absorbent power of the palladium had therefore been reduced to about one-third of its maximum.

The condition of the retracted wire appeared, however, to be improved by raising its temperature to full redness, by sending through it an electrical current from a battery. The absorption rose thereafter to 425 volumes of hydrogen, and in a second experiment to 422·5 volumes.

The wire becomes fissured longitudinally, acquires a thready structure, and is much disintegrated on repeatedly losing hydrogen; particularly when the hydrogen has been extracted by electrolysis in an acid fluid. The palladium in the last case is dissolved by the acid to some extent. The metal appeared, however, to recover its full power to absorb hydrogen, now condensing upwards of 900 volumes of gas.

The effect upon its length of simply annealing the palladium wire by exposure in a porcelain tube to a full red heat, was observed. The wire measured 556·075 m.m. before, and 555·875 m.m. after heating; or a minute retraction of 0·2 m.m. was indicated. In a second annealing experiment, with an equal length of new wire, no sensible change whatever of length could be discovered. There is no reason, then, to ascribe the retraction after hydrogen, in any degree, to the heat applied when the gas is expelled. Palladium wire is very slightly affected in physical properties by such annealing, retaining much of its first hardness and elasticity.

2. *Tenacity.*—A new palladium wire, similar to the last, of which 100 m.m. weighed 0·1987 grammes, was broken, in experiments made on two different portions of it, by a load of 10 and of 10·17 kilogrammes. Two other portions of the same wire, fully charged with hydrogen, were broken by 8·18, and by 8·27 kilogrammes. Hence we have—

Tenacity of palladium wire	100·
Tenacity of palladium and hydrogen	81·29

The tenacity of the palladium is reduced by the addition of hydrogen, but not to any great extent. It is a question whether the degree of tenacity that still remains is reconcilable with any other view than that the second element present possesses of itself a degree of tenacity such as is only found in metals.

3. *Electrical Conductivity.*—Mr. Becker, who is familiar with the practice of testing the capacity of wires for conducting electricity, submitted a palladium wire, before and after charging with hydrogen, to trial, in comparison with a wire of German silver of equal diameter and length, at 10·5°. The conducting-power of the several wires was found as follows, being referred to pure copper as 100:—

Pure copper	100·
Palladium	8·10
Alloy of 80 copper + 20 nickel	6·63
Palladium + hydrogen	5·99

A reduced conducting power is generally observed in alloys, and the charged palladium wire falls 25 per cent. But the conducting power remains still considerable, and the result may be construed to favor the metallic character of the second constituent of the wire. Dr. Matthiessen confirms these results.

4. *Magnetism.*—It is given by Faraday as the result of all his experiments that palladium is "feebly but truly magnetic;" and this element he placed at the head of what are now called the paramagnetic metals. But the feeble magnetism of palladium did not extend to its salts. In repeating such experiments, a horse-shoe electro-magnet of soft iron, about 15 centimetres (6 inches) in height, was made use of. It was capable of supporting 60 kilogs., when excited by four large Bunsen's cells. This is an induced magnet of very moderate power. The instrument was placed with its poles directed upwards; and each of these was provided with a small square block of soft iron terminating laterally in a point, like a small anvil. The palladium under examination was suspended between these points in a stirrup of paper attached to three fibres of cocoon silk, 3 decimetres in length, and the whole was covered by a bell glass. A filament of glass was attached to the paper, and moved as an index on a circle of paper on the glass shades divided into degrees. The metal, which was an oblong fragment of electro-deposited palladium, about 8 m.m. in length and 3 m.m. in width, being at rest in an equatorial position—that is, with its ends averted from the poles of the electro-magnet—the magnet was then charged by connecting it with the electrical battery. The palladium was deflected slightly from the equatorial line by 10° only; the magnetism acting against the torsion of the silk suspending thread. The same palladium charged with 603·6 volumes of hydrogen was de-

flected by the electro-magnet through 48°, when it set itself at rest. The gas being afterwards extracted, and the palladium again placed equatorially between the poles, it was not deflected in the least perceptible degree. The addition of hydrogen adds manifestly, therefore, to the small natural magnetism of the palladium. To have some terms of comparison, the same little mass of electro-deposited palladium was steeped in a solution of nickel of specific gravity 1·082, which is known to be magnetic. The deflection under the magnet was now 35°, or less than with hydrogen. The same palladium being afterwards washed and impregnated with a solution of protosulphate of iron of specific gravity 1·048, of which the metallic mass held 2·3 per cent. of its weight, the palladium gave a deflection of 50°, or nearly the same as with hydrogen. With a stronger solution of the same salt, of specific gravity 1·17, the deflection was 90°, and the palladium pointed axially.

Palladium in the form of wire or foil gave no deflection when placed in the same apparatus, of which the moderate sensitiveness was rather an advantage in present circumstances; but when afterwards charged with hydrogen, the palladium uniformly gave a sensible deflection of about 20°. A previous washing of the wire or foil with hydrochloric acid, to remove any possible traces of iron, did not modify this result. Palladium reduced from the cyanide, and also precipitated by hypophosphorus acid, when placed in a small glass tube, was found to be not sensibly magnetic by our test; but it always acquired a sensible magnetism when charged with hydrogen.

It appears to follow that hydrogenium is magnetic, a property which is confined to metals and their compounds. This magnetism is not perceptible in hydrogen gas, which was placed both by Faraday and M. E. Becquerel at the bottom of the list of diamagnetic substances. This gas is allowed to be upon the turning-point between the paramagnetic and the diamagnetic classes. But magnetism is so liable to extinction under the influence of heat that the magnetism of a metal may very possibly disappear entirely when it is fused or vaporized, as appears with hydrogen in the form of gas. As palladium stands high in the series of the paramagnetic metals, hydrogenium must be allowed to rise out of that class, and to take place in the strictly magnetic group, with iron, nickel, cobalt, chromium and manganese.

Palladium with Hydrogen at a High Temperature.—The ready

permeability of heated palladium by hydrogen gas would imply the retention of the latter element by the metal even at a bright red heat. The hydrogenium must, in fact, travel through the palladium by cementation, a molecular process which requires time. The first attempts to arrest hydrogen in its passage through the red-hot metal was made by transmitting hydrogen gas through a metal tube of palladium with vacuum outside, rapidly followed by a stream of carbonic acid, in which the metal was allowed to cool. When the metal was afterwards examined in the usual way, no hydrogen could be found in it. The short period of exposure to the carbonic acid seems to have been sufficient to dissipate the gas. But on heating palladium foil red-hot in a flame of hydrogen gas, and suddenly cooling the metal in water, a small portion of hydrogen was found locked up in the metal. A volume of metal amounting to 0·062 c. c. gave 0·080 c. c. of hydrogen; or, the gas, measured cold, was 1·306 times the bulk of the metal. This measure of gas would amount to three or four times the volume of the metal at a red heat. Platinum treated in the same way appeared also to yield hydrogen, although the quantity was too small to be much relied upon, amounting to only 0·06 volume of the metal. The permeation of these metals by hydrogen appears, therefore, to depend on absorption, and not to require the assumption of anything like porosity in their structure.

The highest velocity of permeation observed, was in the experiment where four litres of hydrogen (3992 c. c.), per minute passed through a plate of palladium 1 m. m. in thickness, and calculated for a square metre in surface, at a bright red heat a little short of the melting point of gold. This is a traveling movement of hydrogen through the substance of the metal with the velocity of 4 m.m. per minute.

The chemical properties of hydrogenium also distinguish it from ordinary hydrogen. The palladium alloy precipitates mercury and calomel from a solution of the chloride of mercury without any disengagement of hydrogen; that is, hydrogenium decomposes chloride of mercury, while hydrogen does not. This explains why M. Stanislas Meunier failed in discovering the occluded hydrogen of meteoric iron, by dissolving the latter in a solution of chloride of mercury; for the hydrogen would be consumed, like the iron itself, in precipitating mercury. Hydrogen (associated with palladium) unites with chlorine and iodine in the dark, reduces a persalt

of iron to the state of protosalt, converts red prussiate of potash into yellow prussiate, and has considerable deoxidizing powers. It appears to be the active form of hydrogen, as ozone is of oxygen.

The general conclusions which appear to flow from this inquiry are—that in palladium fully charged with hydrogen, as in the portion of palladium wire now submitted to the Royal Society, there exists a compound of palladium and hydrogen in a proportion which may approach to equal equivalents;* that both substances are solid, metallic and of a white aspect; that the alloy contains about 20 volumes of palladium united with one volume of hydrogenium; and that the density of the latter is about 2, a little higher than magnesium, to which hydrogenium may be supposed to bear some analogy; that hydrogenium has a certain amount of tenacity, and possesses the electrical conductivity of a metal; and, finally, that hydrogenium takes its place among magnetic metals. The latter fact may have its bearing upon the appearance of hydrogenium in meteoric iron, in association with certain other magnetic elements.

I cannot close this paper without taking the opportunity to return my best thanks to Mr. W. C. Roberts for his valuable co-operation throughout the investigation.

SPECTROSCOPIC OBSERVATIONS OF THE SUN.

By J. N. Lockyer.

The author, after referring to his ineffectual attempts, since 1866, to observe the spectrum of the prominences with an instrument of small dispersive powers, gave an account of the delays which had impeded the construction of a larger one (the funds for which were supplied by the Government-Grant Committee, early in 1867,) in order that the coincidence in time between his results and those obtained by the Indian observers might not be misinterpreted.

Details are given of the observations made by the new instrument, which was received incomplete on the 16th of October. These observations include the discovery and exact determination of the lines of the prominence spectrum on the 20th of October, and of the fact that the prominences are merely local aggregations

* *Proceedings of the Royal Society*, 1868, p. 425.

of a medium which entirely envelopes the sun. The term chromosphere is suggested for this envelope, in order to distinguish it from the cool absorbing atmosphere on the one hand, and from the white light-giving photosphere on the other. The possibility of variations in the thickness of this envelope is suggested, and the phenomena presented by the star in Corona are referred to.

It is stated that, under proper instrumental and atmospheric conditions, the spectrum of the chromosphere is always visible in every part of the sun's periphery; its height, and the dimensions and shapes of several prominences, observed at different times, are given in the paper. One prominence, 3′ high, was observed on the 20th of October.

Two of the lines correspond with Fraunhofer's C and F; another lies 8° or 9° (of Kirchhoff's scale) from D towards E. There is another bright line, which occasionally makes its appearance near C, but slightly less refrangible than that line. It is remarked that the line near D has no corresponding line ordinarily visible in the solar spectrum. The author has been led, by his observations, to ascribe great variation of brilliancy to the lines. On the 5th of November, a prominence was observed in which the action was evidently very intense; and on this occasion the light and color of the line at F were most vivid. This was not observed all along the line visible in the field of view of the instrument, but only at certain parts of the line which appeared to widen out.

The author points out that the line F invariably expands (that the band of light gets wider and wider) as the sun is approached, and that the C line and the D line do not; and he enlarges upon the importance of this fact, taken in connection with the researches of Plücker, Hittorf, and Frankland on the spectrum of hydrogen—stating at the same time that he is engaged in researches on gaseous spectra which, it is possible, will enable us to determine the temperature and pressure at the surfaces of the chromosphere, and to give a full explanation of the various colors of the prominences which have been observed at different times.

The paper also refers to certain bright regions in the solar spectrum itself.

Evidence is adduced to show that possibly a chromosphere is, under certain conditions, a regular part of star-economy, and the outburst of the star in Corona is especially dwelt upon.—*Chemical News.*

THE TOTAL ECLIPSE OF THE SUN,

August 18th, 1868.

In looking over our late numbers, we find that we have been negligent in keeping our readers informed of the observations made during the unusual phenomenon above named, the reason being, that the news of what had been done and seen at the distant points of observation in Arabia and India, reached this part of the world in such minute fragments, that the proper time to publish a connected account never seemed to come, as something was always wanting to render the story complete.

Now, however, at last we have enough to make a respectable array, and though much is yet to be looked for (no details or drawings by the French party having been published), we will endeavor to put into satisfactory shape the existing material.

Six parties were sent out by various governments, societies, or individuals.

One from the Royal Society, under Lieut. Herschel, located itself at Belgaum, in India; another from the Royal Astronomical Society, under Major Tennant, went to Guntoor, in India, at which place a French party under M. Janssen, who represented the Académie des Sciences, also established itself. Another French party under M. Stephan, posted themselves at Wah-Tonne, on the peninsula of Malacca. While a Prussian corps, under Dr. Vogel, took their ground at Aden, in Arabia, near the mouth of the Red Sea. A few individual observers were stationed at intermediate points, while, lastly, an English party, under Capt. Haig, fixed upon Beejapoor, in India.

Fig. 1 on the plate, shows the location of these various points in connection with the path of the totality, which, in the case of this eclipse, had a breadth of 143 miles, and a maximum duration of nearly *seven minutes*, which is without a parallel in the annals of astronomy.

The special points to which attention was directed by all parties, were—

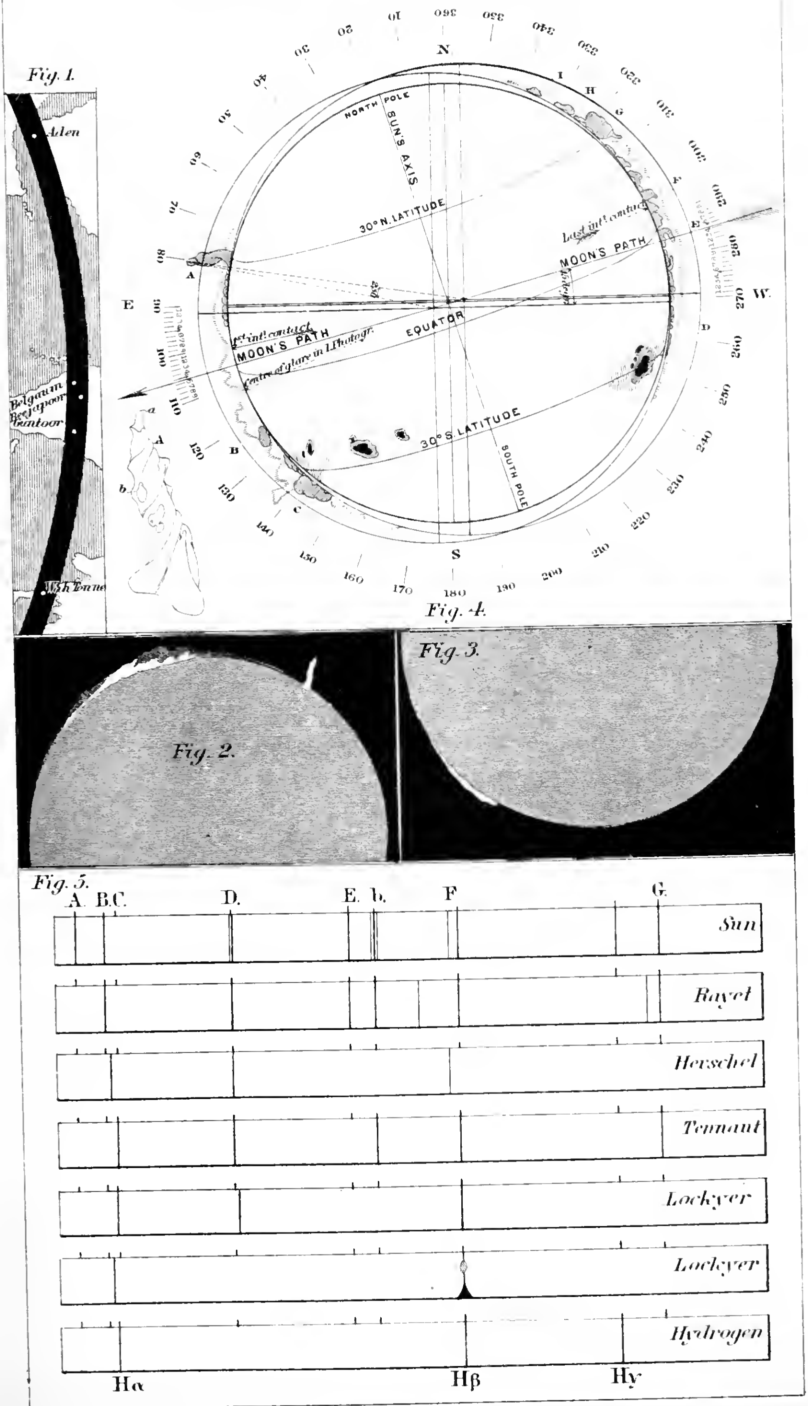
1st. Photographic records of the phenomena.

2d. Observation of the protuberances.

3d. Observation of the corona with the polariscope.

4th. Observation of the corona and protuberances with the spectroscope.

TOTAL ECLIPSE OF THE SUN AUG. 17-18, 1868.



Lith. Jas McGuigan Phila.

The only photographs which have been generally circulated, are those taken by the Prussian or North German expedition, under Dr. Vogel. One of these was sent by this gentleman some time since, to Mr. E. L. Wilson, editor of the *Philadelphia Photographer*, and other copies are to be placed in his hands for sale. We hope in our next number to give copies of these photographs, but for the present will only refer to the outlines Figs. 2 and 3, on the plate, which will serve to indicate the general features shown in these pictures, and which we have copied through the kindness of Mr. Wilson, from those first sent to him.

Figure 2 indicates the objects seen in the plate exposed at the first moment of totality. These consist chiefly of a long series of flame like prominences, stretching for a distance of at least 500,000 miles along the edge of the sun, and composed of more brilliant central masses, with tongues and fringes of fainter light The impression which this photograph conveys, is of a vast conflagration, blown by a wind from left to right, and seen over the edge of a serrated range of hills. The flame-like appearance of these luminous protuberances, must, however, be regarded only as an accidental similarity in shape, as there are many reasons which make the existence of anything like true combustion in these regions extremely improbable.

These luminous bodies, as we shall presently see from the results of the spectroscopic observations, are undoubtedly cloud-like masses of intensely hot, but not burning, gas; of gas which in fact is far hotter than any combustion could make it.

Following round the solar and lunar edge to the right, we then encounter a tower-like mass of similar luminous matter, which on comparison with the solar radius, is found to be nearly 70,000 miles high, and about 10,000 miles across the base. Assuming, as we have good reason to do, that its other dimension is about the same, we find that its entire volume would approach 7,000,000,000,000 of cubic miles, or 27 times the earth's entire bulk, or enough to cover us in to the depth of some 8,000 miles, with an atmosphere of a constant density, our own atmosphere under the same condition being only sufficient to reach about 5 miles.

Fig. 3 represents the picture obtained by Dr. Vogel and his party during the *last* moments of totality; it shows a range of low-lying luminous cloud-matter, ill defined and with no remarkable characteristic. The weather at this station was very unfavorable, the

sky being covered with clouds, as is proved by a picture taken a few minutes after the totality, in which the crescent sun is seen enveloped in, and almost obscured by thick clouds.

The pictures taken by Major Tennant, were, according to his own first account, very unsatisfactory, being "under-exposed, and covered with spots." On subsequent examination, however, he found the negatives better than he had expected, and from a set of prints which he transmitted to Mr. Warren De la Rue, that gentleman has prepared and published in the Monthly Notices of the Royal Astronomical Society, the drawing, of which, Fig. 4, is a slightly reduced copy. Major Tennant also sends an enlarged drawing of the tower-like projection already described, which he names appropriately a horn, and in which his pictures show unmistakable evidence of a spiral structure. A *a b* in the left-hand lower corner of Fig. 4 is a copy from this. A similar spiral structure is shown in one of the prominences taken by De la Rue in the eclipse of 1860.

Major Tennant estimates the height of this great horn or spiral as 90,000 miles.

Mr. De la Rue from a comparison of various drawings, published in the *London Engineer*, supposes that this horn was rotating about its axis. Thus, the difference in actual time between the appearance of the eclipse at Aden and Guntoor was forty minutes; during which a semi-rotation seems, by the pictures, to have occurred; so in the forty-nine minutes between Guntoor and Wha-Tonne, another half turn, and so on, with decreasing velocity.

On Fig. 4, have been introduced in their proper places, the sun-spots present at the date of the eclipse, as shown by the photographs regularly taken at the Kew Observatory. No other connection appears to exist between them and the luminous protuberances, except that both are excluded from the circum-polar regions.

This drawing also shows how entirely the solar disk was overlapped by that of the moon, which was of course the reason of the great breadth of the shadow, and the unusual duration of the totality.

Observations with the polariscope upon the corona, are thoroughly accordant from all stations, and indicate that the light was polarized in plains passing through the sun's centre, as it should be if it consists simply of light reflected by particles of matter surrounding the sun.

Observations upon the corona, with the spectroscope, either gave

no result, or showed a very faint and apparently continuous spectrum.

The luminous prominences, however, showed themselves to be gaseous, and gave marked spectra, in which the various lines delineated in Fig. 5, were recognized by the different observers, as there set down. The lines upon which nearly all observers seem to agree are. A red line corresponding with Fraunhofer's line C, a yellow one at or near D, a blue one coinciding with F, and a violet one corresponding with G.

The correspondence of three of these lines with those derived from the light of hydrogen heated by an electric discharge, is very marked. Thus, as Plucker, by whom this subject was developed, states (*Phil. Trans.* 1865, p. 20, Note). The red line of hydrogen coincides with C, its blue line with F, and its violet with a marked line a little short of G. (See Plate, Fig. 5, last spectrum.)

It would thus seem to be extremely probable that hydrogen is a main constituent of these prominences, a fact which is further established by other circumstances. Thus, hydrogen, when so heated as to give out light resolvable into the three lines mentioned as above, and indicated in Fig. 5, has a brilliant rose color, which agrees with the tint observed in the solar prominences. Again, Lockyer, when studying the spectra of these prominences without the aid of an eclipse, in the manner described by us on p. 87, found that the blue line corresponding to Fraunhofer's F, was much expanded at its base, or part nearest the sun. (See Fig. 5, spectrum next to last.) This would naturally be its hottest part. Now, on turning to Plucker's paper in the *Philosophical Transactions*, in which the experiments with gases, heated in tubes by the electric discharge and observed with a spectroscope, are recorded, we find (*Phil. Trans.*, Vol. 155, p. 21), "Hydrogen shows in a most striking way the expansion of its spectral lines and their gradual transformation into a continuous spectrum. When the direct discharge of Ruhmkorff's large induction coil is sent even through the old spectrum tubes, enclosing hydrogen, the formerly obtained spectrum is essentially altered."

"By increasing the power of the coil, the violet line $H\gamma$* first expands; while it continues to expand, the expansion of the bluish line, $H\beta$, becomes visible."

* These symbols, H_α, H_β, and H_γ are used by Plucker to indicate the lines in hydrogen corresponding to C, F, and the one near G in the solar spectrum. (See Fig. 5, last line.

"Let the aperture of the slit be regulated so that the sodium line will separate into two single lines nearly touching each other. Then, the angular breadth of $H\beta$ becoming two or three minutes, the breadth of $H\gamma$ is about double. The expansion takes place as well towards the less as towards the more refracted part of the spectrum, $H\alpha$, remains almost unchanged after $H\gamma$ has passed into an undetermined large violent band, and $H\beta$ extended its decreasing light on its two sides. On employing the Leyden jar, and giving to the gas in our new tubes a tension of about sixty millions, the spectrum is already transformed into a continuous one, with a red line at one of its extremities."

From this, it is not only obvious that the temperature of these protuberances is greater in one part than in another, (the highest heat, as is natural, being in the part nearest the sun, but also that the peculiar action of their light under this condition corresponds with that of hydrogen; for though a similar expansion of lines takes place with other gases under the influence of intense heat, in none is it so marked or so regular as with hydrogen.

As will be seen in an abstract published in this number, p. 266, Lockyer proposes to make this point a special subject for investigation, and very interesting results may be confidently expected.

Besides these lines, indicative of hydrogen, most observers also agree as to the presence of a yellow band in the place of D of the solar spectrum. This would indicate vaporized sodium. Lockyer in his study of the prominences, without the eclipse, states that appearances indicated the difference in constitution of various prominences. Thus, he occasionally observed a new red line below C, as shown in Fig. 5.

By his method of observation, the comparison of lines and determination of position is specially easy, for, as will be seen by reference to p. 87 of this *Journal*, the spectrum of the prominence is seen in contact with a faint solar one, so that the coincidence of lines may be recognized with the utmost facility. Observing in this way, he reports that the yellow line does not correspond with D, but is distant from it by eight or nine degrees of Kirchhoff's scale.

Several other lines were recorded by M. H. Rayet, about which it would hardly be safe to hazard any conjecture, except in the case of one also observed by Major Tennant. This was of a green color, and corresponded in position with *b* of the solar spectrum. A simi-

lar line is developed both by iron and magnesium when vaporized. The line coincident with E, seen by M. Rayet, might also be due to the former metal.

We look with great interest for the account of the observations made by the French party at Guntoor. They seem to have enjoyed admirable weather, and if their photographic arrangements and manipulations were good, they may have secured some very important results. Thus it will be remembered that in the photographs of the total eclipse of 1860, taken by Warren de la Rue (*Phil. Trans.*, Vol. CLII., p. 333) a number of prominences were shown in the photographs which were not seen by the observers with the telescope, even in places where especial examination had been made by reason of some peculiarity in the objects noticed.

In the last eclipse, we have, however, as yet heard of nothing of this kind. This is quite explicable, if we consider that the German photographers, who managed their process with the greatest skill, had the most unpropitious weather, the sky being full of clouds, which must have absorbed every trace of the invisible but actinic rays, while the English party at Guntoor, who had admirable weather, allowed their baths to be concentrated by the heat of the climate, and over-exposed all their pictures.

It is much to be lamented that Mr. De la Rue adhered to his vow, made during the former eclipse, of not attempting instrumental observations of another, and it is to be devoutly hoped that though our Government has refused any assistance towards the observation of the eclipse which will be visible in this country next August, some public institutions or private individuals will be excited to imitate the above mentioned gentleman, who, in 1860, expended $1,500 of his own funds, in the necessary preparations and conduct of his observations, although in his case, a Government appropriation was made of all that he named as the estimated cost, and a ship of war was put at his disposal for the transportation of his material and assistants.

The eclipse next August will be total over a belt 100 miles in width, and running from Alaska to Beaufort, North Carolina. It will occur on the 18th, about noon, at Alaska, and near sunset, at North Carolina.

EDUCATIONAL

SUNLIGHT AND MOONLIGHT.

A Lecture delivered at the Academy of Music, before the Franklin Institute, on May 23d and June 6th, 1868.

BY PROF. HENRY MORTON, PH. D.

(Continued from page 208.)

IN the preceding description of a sun-spot, mention was made of the "faculæ," which are appearances we have not yet described.

When the general surface of the sun is observed with a moderate magnifying power, it is seen to be of irregular brightness, the appearance being such as we can perhaps best explain by describing at once the object and its probable cause.

The idea suggested most naturally to the observer, is that he is looking at an expanse of variegated white cloud, rolled over in a long irregular ridge in one place, heaped into a broader mass at another, and composed of intersecting ridges at yet another point. These more luminous parts are called "faculæ," and it seems reasonable to believe that they are in fact what they appear to be, accumulations of that luminous cloudy matter of which the visible solar surface consists. These faculæ are especially distinct near the edge of the sun's disk, and in the vicinity of sun spots, and are often seen after the disappearance of a spot round the sun's edge, as in the case mentioned on p. 200. Besides these larger objects, the entire surface not occupied by spots has a mottled appearance, as though covered with luminous specks, which have been variously named "willow leaves," "rice grains," "granules," &c., according to the resemblance suggested to the minds of different observers. These markings are of the most irregular figure, as would appear from the testimony of the majority of observers, although some have defined them as possessing a marked regularity in outline, and one sober and discreet astronomer, no other than Sir John Herschel, has even hazarded so wild a conjecture as the following. He says, after describing them as the sources to us of the sun's light and heat, "Looked at in this point of view, we cannot refuse

to regard them as *organisms* of some peculiar and amazing kind; and though it would be too daring to speak of such organization as partaking of the nature of life, yet we do know that vital action is competent to develop both heat, light and electricity. These wonderful objects can hardly be less than a thousand miles in length, and two or three hundred in breadth." (Lectures on Scientific Subjects, p. 84.)

As a rule, these granules are of an elongated shape, and are arranged with no order as regards their direction; but around the edges of spots they generally point with their longer axis towards the centre of the spot, and give a thatch-like appearance to the penumbra, as is indicated in Figs. 4 and 5 of Plate VII., facing p. 206. On some occasions, one or more of these granules has been seen to break loose, and as it were, set sail across the dark portion of a spot.*

The most probable theory as to the nature of these objects, seems to be that which we have before noticed, and which describes them as cloud-caps at the summit of rising streams of vapor, where the material of which they are composed has reached a sufficient condensation to be a source of white light.

There have also been observed on the less brilliant spaces between these white granules, numerous minute dark points like stiplings with a soft lead pencil. These have been called "pores," by Sir John Herschel, and "punctulations" by his father. They are too minute to enable us to reach any conclusion as to their meaning.

Before leaving this class of phenomena connected with the solar surface, we should say that the abundance of sun spots varies in regular periods from a maximum in frequency and extent, through a minimum to a new maximum, in a period of eleven years, coinciding with a like period in the occurrence of auroræ and magnetic disturbances experienced in the earth, and appearing to be so related to the motions of the planets Venus and Jupiter (the first being effective by reason of its propinquity and the second by its mass), that it is but reasonable to suppose that the presence of these planets is influential upon this solar action.

(To be continued.)

* *Monthly Notices, Royal Astronomical Society*, 1865, *p.* 236.

LECTURES ON VENTILATION.

By Lewis W. Leeds.

Second Course, delivered before the Franklin Institute, during the winter of 1867–68.

(Continued from page 204.)

Now, I think we should be able to apply these *principles* to the heating and ventilation of rooms.

We must study to comprehend the principles, because the circumstances surrounding us are varying so constantly that such arrangements as would suit us best at one time, would not be good at another.

A primary condition to be observed in all heating and ventilating arrangements, as before alluded to, is to keep the feet warmer than the head, and the back warmer than the face.

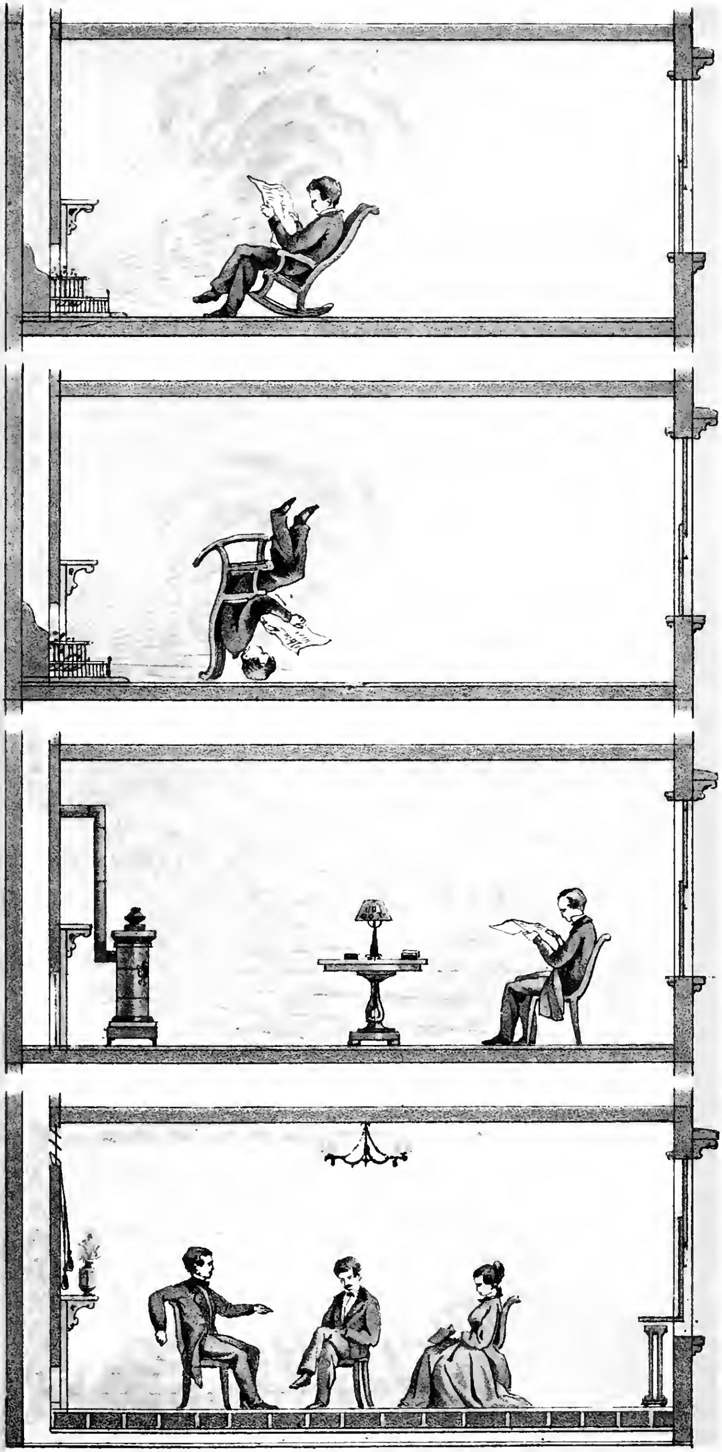
These are great natural provisions, as in walking, or when in motion we face the current. We inhale from the air in front of us, and the fouled air is carried behind us. Therefore, for better protection, probably the greater portion of the nerves are concentrated near the spine; consequently, if the back is chilled, the whole system is put out of order; and more care is required to keep the feet warm, because, being farthest from the heart, the blood is more liable to become chilled, but if the extremes are kept warm, of course the intermediate parts must necessarily be so.

The sun's rays falling upon the earth's surface, and heating our feet hotter than the air around us, is a beautiful illustration of this principle.

Nearly all animals except man, secure this condition by building their houses, or burrowing below the ground, so that the warmth of their bodies causes the foul air to flow out, while the cool fresh air to supply its place, must come in from above, and consequently fall upon their heads first.

It must be admitted that most of our arrangements for ventilation and warming do not fully meet these requirements; indeed, a careful examination will reveal the fact that many of them are as absurd and unscientific as they can well be made.

I have here a diagram (see lithograph Plate, Fig. 1), which has been prepared to represent the condition of different parts of the room, when heating exclusively by open fires.



I have been unable to invent or think of any simple method of showing the actual passing or currents of radiant heat.

It is one of the most difficult points to comprehend in connection with the subject of heating and ventilation. It is also one of the most important.

In this diagram, we have resorted to colors to express our ideas. By the blue is designated the strong current of cold air that is always found flowing along the floor from the windows and doors towards the open fire; and of course the feet and back of those sitting with their faces to the fire, are most affected by this cold air. It may at times be but little above the freezing point; but directly in opposition to this current there is a strong radiation from the bright fire, directly into the face and front of those sitting before it.

We must remember the direct radiation from an open fire or other hot body is not disturbed by the current of air.

Now to get his back warmer than his face, and his feet warmer than his head, it will be necessary to change his position. (Fig. 2.)

I admit, this is rather an awkward position. You know the conventional way for an Englishman to represent a Yankee, is to have him tilted back in a chair, with his feet on the mantel, or in some position higher than his head.

So you see there is a little more practical common sense about this position of the universal Yankee, than it would at first appear.

Now, I do not suppose this will become a fashionable way of sitting, even if the physiologists were to recommend it. But if we want to be really comfortable and healthy, we shall have to change either our manner of sitting, or our manner of heating and ventilation.

Let us consider this open fire question a little carefully, because there are some very good points about it. The heat derived from actual combustion, that is from an open fire, is the purest artificial heat we can possibly have. The temperature of the burning coals or wood is over 3,000°, and the flow of radiant heat therefrom more nearly imitates the sun than from surfaces of a lower temperature.

This is the strongest point in its favor. It is also impossible to prevent its being a good ventilator. And this is no small account to its credit, as it is almost the only ventilation that the people, in their ignorance, do not stop up.

But it also has its disadvantages, which have prevented, and always must prevent its coming into universal use for heating exclusively.

One of these is represented by the diagram in the very uneven manner of distributing the heat, and as it heats the head hotter than the feet, and the face hotter than the back, we shall have to ignore it altogether, or what I think will do better, get some additional assistance partially to warm the air before it enters.

Let us now consider the great abuse of the system of heating by direct radiation. We have prepared some tanks for using in the lantern, and by using liquids of different colors and densities, I think we shall be able to express to you some of the principal movements of air of different temperatures.

If you will notice carefully, you will soon see that the liquids used to night are governed by the same laws, and move in almost precisely the same manner, as the air and gases of different densities that we used the other evening.

Professor Morton has very kindly offered to assist me this evening, and has now placed in the lantern a small tank, and by the aid of the new Zentmayer prism, shall be able to show you the image on the screen, just in its natural position.

This we could not have done last year, without the aid of this prism, and as we would have had to work everything bottom upwards, I did not then use the lantern for this purpose.

At the left hand corner of our room, (Fig. 3), you see the shadow of a small coil, which is fine platina wire. Professor Morton will connect the two ends of that coil with a small battery, and thus put a current of electricity through it, by which it will be heated, and you will very soon see the liquid around that coil (represented by stove in the figure) beginning to ascend. And now you see, as the air around the stove commences to rise, the air from below flows in to take its place, thus giving a revolving motion to all the liquid in the room.

You also notice the figure of a man sitting very quietly, with his back to the window, as though he was in a profound meditation.

We will have to disturb his quiet a little, I think.

(*Here the figure was made to breathe.*)

You see, the breath being loaded with carbonic acid and moisture, falls directly to the floor, and mingling with the revolving mass, will soon be carried quite around the room, and back again to be re-breathed

Now, here is a combination of warming by direct radiation, and warming the air also by its coming in contact with the stove.

Now please take especial notice of the conditions here represented. This applies to a large number of our ordinary buildings heated by steam, when the coils are placed directly in the room.

It also very nearly applies to rooms heated by air-tight stoves, as there is so little air consumed in them to support combustion, it is hardly worth noticing as ventilation.

The air is already becoming very foul, as it inevitably does, in rooms treated in this manner, when no attention is given to having a regular supply of air.

It allows the room and all its inmates to be sealed up perfectly air-tight. It not only *allows* it to be done, but we generally find that advantage is taken of this opportunity to close all cracks, and it is most zealously accomplished.

There is one other point of much practical importance represented in this: that is, in sitting with the back towards a cold window; the additional warmth or animal heat of the body will be radiated to and absorbed by this cold window. And thus the very being, life and vitality will be abstracted, and spinal disease, rheumatism, and other disorders, are the results frequently of continued sitting in this position. Therefore, never sit with your back to a window. It will be observed, that the cooling of the air by its coming in contact with the window, by no means purifies it.

Now, this is the great *abuse* of direct radiation; the way a large proportion of our buildings and rooms are arranged. I think we can make an improvement on this. (Fig. 4.)

In the first place, the heating surface is represented in this case as a coil of steam pipe but it may be an open fire or stove or any other heating arrangement, and is placed on the cold side of the room and directly under the window, which, being but thin glass, is generally the coldest part of the room.

Now, by raising the window a little, there will be a current of fresh air blowing directly against this heating surface; the entering air will thus be partially warmed. It need not be warmed to the temperature of the room, however, because there will be a large proportion of the heat derived from the direct radiation of the exposed heater.

This gives the very great advantage of having cool, invigorating air for breathing, while the body is warmed by the direct rays from

the fire or other heating surface. It also prevents those cold, unpleasant draughts so commonly complained of near windows.

The temperature of the fresh air, too, being so near the required temperature of the room, has no special tendency to rise immedidiately to the ceiling and escape through any opening that may be made there. On the contrary, being rather cooler, it is rather inclined to remain near the floor, except when it is additionally warmed by the persons, gas-lights, or some extra heating surface. The ventilation at the top of the room can always be left open without inconvenience, with this arrangement; this, too, is a very great advantage, as it should always be open in the evening under all circumstances, to carry off the products of combustion from the lights; and it is so difficult to have it opened in the evening and closed in the day time, with our present careless and indifferent notions about ventilation.

Therefore, an arrangement not requiring this daily attention, is valuable.

The figure represents an additional foul air channel under the floor. The air is represented by the arrows being drawn from the floor of the room around the base-board, or in schools or meeting-rooms when large numbers are assembled—from numerous points over the floor.

This frequently for large, crowded rooms, is an important arrangement. Of course these general plans will require many modifications in practice. But this combination of direct radiation and circulation of partially warmed air, and the placing of the heating surfaces on the cold sides of the room, and especially under the windows, will be found to be a hint of much value.

(To be Continued.)

ON THE FUTURE DEVELOPMENT OF SCIENTIFIC EDUCATION IN AMERICA.

By S. Edward Warren, C. E.

Prof. of Descriptive Geometry, &c., in the Rensselaer Pol. Inst., Troy, N. Y.

(Continued from page 212.)

Each single institution would thus be regarded, not as an independent unit, but as a *free* component unit; first, of the entire assemblage of institutions of its own class; and, second, of the grandest unit, the aggregate of all the associated higher institutions of the land.

Each institution, and each enthusiastic thinker on the subject of higher education, would thus be emancipated from the idea that formal unity could be realized only through the appalling unwieldiness, consequent upon crowding instruction in every ancient and modern study, both general and technical, too, into a single institution. The original scheme of the "University of the South," with its thirty-two "Schools," so-called, was a marvellous example of this bombastic method of realizing the idea of unity. If we mistake not, there is a tendency to something of similar unwieldiness among some other and more prosperous institutions, arising probably from ambition not governed by some adequate regulating idea. Each institution would thus be happily free, both in conviction and feeling, to act on the wholesome principle, that the most finished results are to be obtained, by concentration of interest and effort upon a limited field, which should be more thoroughly cultivated.

We only add here, briefly, two more considerations.

First. The rehearsal, in new terms, of a cardinal principle already explained, viz., that general, or "liberal," and technical, or "professional" education are not rival competitors, on the same plane, as might be inferred from the large attendance on all professional schools, by those who are not college graduates; but that the latter are the natural successors of the former. Hence, that while each has its prevailing immediate end, by which it is distinguished, yet they are united by a general common end. That is, *proficiency in professional practice* is the *immediate* end of professional study, but the *remote*, though no less real end of general or collegiate study. In other words, a student would pursue a preliminary course in general science in order that he might become a better *student* of engineering, for example. He would *then* enter an engineering school, in order to qualify himself for the *immediate practice* of his profession.

Second. All men are self-educated who are educated by any activity at all of their own. The true alternative is, shall they educate themselves with, or without the helps afforded by experienced teachers? The obvious answer is, of itself, the grand justification of the existence of all institutions for purposes of *formal* education. But every influence in life that has any effect upon man is, so far, an educating influence. There is, then, an *informal* or unconscious education going on through every conscious moment of existence,

in the schools of the home, the church, the street, the shop, &c.; and this is why a programme of *formal* education need not embrace every educating element under the heads of observation, experiment, reflection and practice, upon and with Man and Nature.

Curriculum, and remarks thereon.

TOTAL COURSE = Five Years.

General Course = Three Years.

Technical Courses = Two Years each, in

Civil Engineering. {
Construction Engineering,
Mechanical Engineering,
Mine Engineering,
Industrial Architecture.
}

Remarks (*A*).—According to the principles of unity already laid down, we cannot be clear in uniting more than these four technical courses in a single institution. Some persons, indeed, would perhaps prefer, and not without reason, that but one technical course should be given in any one institution, after a general course shaped with reference to that one. This would, indeed, secure the greatest simplicity of composition and concentration of energies on the simple principle of undertaking but one thing, and doing that one perfectly. But the four technical courses just named, are so closely related—three of them as merely subdivisions of *Civil*, as opposed to *Military Engineering*, and all, as based alike on the sciences of *Number* and *Form*, and they require so very much in common, in their preparatory general course, that there seems to be no decisive objection to assembling them in one school. To go further than this, would require nothing less than the study of the whole of Nature, in relation to Man's material wants, and would admit schools of *Topographical Engineering; Technical Natural Science* for the training of professional *Naturalists*, *Geologists*, *Physicists*, and *Chemists;* schools of *Industrial Ornamental Design*, of *Commerce and Trade*, of *Agriculture*, and of *Industrial Technology* as applied to the mechanical trades.

An institution embracing all the schools thus far mentioned would be what we have sometimes fancied as an all-embracing *Polytechnic University*, in distinction from the equally broad *Humanistic University*, in which man and his personal wants should be the distinctive subject of study, thus giving rise to schools of *Law*, *Medicine*, *Divinity*, *Philology*, *Sociology*, and *Higher Teaching.*

But never, in the highest flights of ambitious fancy, could we imagine the union of these two universities in one to be desirable, so radical is the distinction between them. Yet, in the absence either of perception of, or regard for such a limiting distinction, the engrafting of schools of engineering, &c., upon classical colleges, looks to the supposed possibility of a university in which absolutely every branch of human knowledge and pursuit should be taught. It is not to be forgotten, however, that the great bequests often given to institutions may be coupled with conditions, whose fulfilment may conflict with the ideal views of higher educators.

We have already shown that it is not necessary, in order to realize the idea of a comprehensive unity in the higher seminaries, that each should teach everything. It is not even indispensable that there should be formal organizations, embracing the instructors in them. It is sufficient that what is *predominant in any of them* should have at least a *rudimentary representative in all;* according to the teaching of Nature in flowers, for example, where it often happens that organs, as the calyx, corrolla, or stamens, which are fully developed in one flower, exist only in a rudimentary form, or invisibly, in another. Thus it would probably be, best that a *University of Fine Arts*, to embrace thorough and professional training in *Music*, *Painting*, *Sculpture*, *Ornamental Architecture*, *Oratory*, and *Higher Gymnastics*, should be separate from the two species of Utilitarian Universities already described. Thus the *art idea* and *sentiment*, which would characterize such a University, would find its diminished representative to grace the life of *different* institutions in *their* possession of mere voluntary associations for music or field sports, of a *few* fine paintings and statues scattered through their halls, and of tasteful grounds.

(*B.*)—A course in *topographical engineering* is omitted from reasons of expediency simply. For, until the separate States of the Union institute state topographical surveys, the comparatively few engineers required on the coast survey can be supplied from the national institutions.

(*C.*)—The erection of fully equipped machine-shops, as a part of the means of instruction in schools of mechanical engineering, seems to us inexpedient. The regulating idea seems to be, that school education is an education primarily in *principles*, while that of the shop is a training in *processes*. Hence, while it doubtless would be well to keep a few standard working machines, and a

skilled mechanic to operate them, and to have students of mechanical engineering *see* the operations and have them explained, and be thoroughly interrogated upon them, it seems unnecessary, as it certainly would be impracticable, to make a skilled operative mechanic of every professional mechanical engineer. In addition, however, to seeing and noting the operations of machinists' tools, the student of mechanical engineering should have full opportunity, through a cabinet of machines and elementary combinations, to study the construction and action of the most important machines; always remembering that the profound study of a few illustrations confers more practical power than the shallow study of many.

(*D.*)—*Civil Engineering*, as opposed to *Military*, is a generic term, having, through the complexity of modern life, many separate departments, as has the Law.

Construction Engineering (so called for want of a better term, though *this* has precedent in high French authority,) might have been further subdivided into Road, Hydraulic, and Naval Engineering. The recognition of these as distinct departments, in a system of school instruction, would be only a question of time, depending on expediency.

Provision of Time.—Let the scholastic year, after deducting all vacations and holidays, yield 39 weeks of working days, in three equal sessions of 13 weeks each, from which further deduct 9 weeks, 3 from each session, for the principal reviews and examinations, leaving 30 weeks, at 5 days to the week, $=150$ days. And let each student be present on duty, in theoretical and practical exercises, united, 5 hours daily, on an average, giving 750 hours per year as a maximum.

The granting of the whole of Saturday, every week, to the student, amply and duly provides for the *voluntary element in student life*, and thus enlivens and refreshes and contents it.

It also affords a most useful leeway, in which to equalize the products of unequal capacities and of unequal opportunities arising from necessary absences. The most capable can always find some profitable way of spending its hours, and, to the most hindered, it affords the great privilege of making up arrears before they can accumulate so as to become unmanageable.

And, best of all, it removes all excuse for neglect or waste of the invaluable privileges of Sunday, for the highest self-culture, and readjustment for the immediate future, through instruction and reflec-

tion upon the best, greatest, and most enduring things of both the past and the future.

With a liberal admixture of practical exercises, not requiring severe mental labor, and regarding field practice in Natural History and Geology as especially healthful, 5 hours daily for close study at home are contemplated, leaving 14 hours daily for sleep, meals, society, long or short recreations, and practical work at home.

(To be continued.)

Franklin Institute.

Proceedings of the Stated Monthly Meeting, Feb. 17th, 1869.

THE meeting was called to order, and in the absence of the President and Vice-Presidents, Mr. Samuel V. Merrick took the chair on motion.

The minutes of the last meeting were read and approved.

The Actuary submitted the minutes of the Board of Managers, and stated that the New Board, organized on the 27th January inst., by appointing the following Standing Committees for the ensuing year:—

1. *On Instruction.*

Henry Morton,
Coleman Sellers,
Robert E. Rogers,
Enoch Lewis,
Caleb S. Hallowell.

2. *On Elections and Resignations.*

O. Howard Wilson,
Henry G. Morris,
Henry Cartwright,
Samuel Hart,
Charles S. Close.

3. *On Stocks and Finance.*

William Sellers,
Frederick Fraley,
James S. Whitney,
James Dougherty,
Washington Jones.

4. *On Publications.*

Coleman Sellers,
Bloomfield H. Moore,
Samuel Sartain,
Charles Bullock,
Pliny E. Chase.

Messrs. Bloomfield H. Moore and Charles Bullock were elected Curators for the ensuing year.

The following resolution was adopted, viz:—

Resolved, That the Committee on the New Hall be requested to consider the propriety of soliciting from the Legislature of Pennsylvania the right to occupy the south-western angle of the Penn

Squares in this city, for the purpose of erecting thereon a New Hall, with power to act; the assent of the Institute being first obtained.

At the stated meeting held February 10th inst., donations to the Library were received from the Royal Astronomical Society, the Chemical Society, and the Society of Arts, London, England; l' Academie des Sciences, Paris, and la Société, Industrielle, Mulhouse, France; the Water Commissioners of Jersey City, New Jersey, and Professor Henry Morton, Philadelphia.

The various Standing Committees presented their minutes.

The regular report of the Resident Secretary on Novelties in Science and the Arts was then read.

New business being then in order, it was on motion

Resolved, That the Committee on the New Hall be directed to make applications to the Legislature for the use of one of the Penn Squares for the erection of a building.

It was also on motion—

Resolved, That the President and Secretary be requested to make application to the Senate and House of Representatives of the United States for the appropriation of funds to provide for observations of the total eclipse of the sun in August next. The money to be expended under the direction of the Chief of the Bureau of Navigation, or of the Superintendent of the Coast Survey.

After which, the meeting on motion, adjourned.

HENRY MORTON, *Secretary.*

Complementary Colors by Reflected and Transmitted Light, are admirably shown by a simple arrangement, to which our attention has been called by Prof. C. Pickering, of Boston. A plate of glass is coated with a layer of the violet colored ink, made from aniline color, now much used, and this fluid is allowed to dry upon it. If we then place this in such a position that light is reflected from its surface to our eyes, it will appear of a metallic golden color, as though coated with a gold bronze; but if we look through it at the light, the color will be a very rich purple. There are many other bodies having a similar action, but in none that we know of, is it so striking as in this. Thus, glass flashed with silver, has a green color by reflected, and an orange red by transmitted light. Salts of the sesquioxide of chromium, which are green by reflected are red by transmitted light, a solution of ordinary litmus is blue by reflected but red by transmitted light.

A COMPARISON *of some of the Meteorological Phenomena of* FEBRUARY, 1869, *with those of* FEBRUARY, 1868, *and of the same month for* EIGHTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11¼′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	February, 1869.	February, 1868.	February, for 18 years.
Thermometer—Highest—degree	61·00°	52·00°	70·00°
" date	13th.	20th.	23d, '60.
Warmest day—mean	50·83	42·67	59·30
" " date	13th.	21st.	25th, '57.
Lowest—degree	15·00	3·00	—1·00
" date	28th.	23d.	7th'55;8th,'61
Coldest day—mean	22·17	12·83	—4·50
" " date	28th.	23d.	6th, '55.
Mean daily oscillation	12·21	16·00	13·53
" " range	5·27	7·84	7·10
Means at 7 A. M.	34·04	21·72	29·58
" 2 P. M.	41·79	31·21	38·49
" 9 P. M.	36·91	26·72	33·80
" for the month	37·58	26·55	33·96
Barometer—Highest—inches	30·457	30·748	30·970
" date	25th.	1st.	11th, 67.
Greatest mean daily pressure	30·402	30·672	30·862
" " " date	25th.	23d.	11th, '67.
Lowest—inches	28·984	29·566	28·984
" date	4th.	6th.	4th, '69.
Least mean daily pressure	29·235	29·633	29·227
" " " date	4th.	28th.	16th, '56.
Mean daily range	0·296	0·278	0·232
Means at 7 A. M	29·908	30·197	29·947
" 2 P. M	29·886	30·154	29·902
" 9 P. M	29·931	30·156	29·932
" for the month	29·908	30·169	29 927
Force of Vapor—Greatest—inches	0·367	0·231	0·549
" date	15th.	21st.	16th, '57.
Least—inches	·047	·038	·013
" date	28th.	23d.	6th, '55.
Means at 7 A. M	·158	·100	·140
" 2 P. M	·161	·121	·157
" 9 P. M	·160	·121	·157
" for the month	·160	·114	·151
Relative Humidity—Greatest—per cent	96·0	95·0	100·0
" date	15th.	27th.	Often.
Least—per cent	29·0	42·0	20·0
" date	18th.	19th.	22d, '64.
Means at 7 A. M.	75·1	80·0	78·3
" 2 P. M.	58·1	67·1	63·0
" 9 P. M.	70·4	79 5	75·0
" for the month	67·9	75·6	72·1
Clouds—Number of clear days*	8·	11·	8·4
" cloudy days	20·	18·	19·7
Means of sky covered at 7 A. M	60·0 per ct	64·5 per ct	60 8 per ct
" " " 2 P. M	59·6	50·7	60·5
" " " 9 P. M	43·2	47·9	48·3
" " for the month	54·3	54·4	56·5
Rain and Melted Snow—inches	4·49	2·59	3·306
No. of days on which rain or snow fell	12·	6·	9·9
Prevailing Winds—Times in 1000	S59°52′W.244	N77°48′W.240	N75°41′W.271

* Sky one-third or less covered at the hours of observation.

A COMPARISON *of some of the Meteorological Phenomena of the Winter of* 1868–69, *with that of* 1867–68, *and of the same Season for* EIGHTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11¼′ W. from Greenwich. By JAMES A. KIRKPATRICK, A.M.

	Winter, 1868–'69.	Winter, 1867–'68.	Winter, for 18 years.
Thermometer—Highest—degree	61·00°	52·50°	71·00°
" date	Feb. 13.	Dec. 27.	Dec. 2, '59.
Warmest day—mean	51·67	47·33	62·80
" " date	Jan. 9.	Dec. 28.	Dec. 19, '56
Lowest—degree	13·00	3·00	—9·00
" date	Dec. 25.	Feb. 23.	Jan. 8, '66.
Coldest day—mean	20·33	12·83	—1·00
" " date	Dec. 24.	Feb. 23.	F·7'55; F8'61
Mean daily oscillation	11·17	13·21	12·47
" " range	5 11	6·60	6·62
Means at 7 A. M.	32·84	26·27	29·74
" 2 P. M.	38·87	32·86	37·39
" 9 P. M.	35·14	29·45	33·09
" for the Winter	35·62	29·53	33·40
Barometer—Highest—inches	30·489	30·748	30·970
" date	Dec. 13.	Feb. 1.	Feb. 11, '67.
Greatest mean daily pressure	30·415	30·672	30·862
" " " date	Dec. 13.	Feb. 23.	Feb. 11, '67.
Lowest—inches	28·984	29·291	28·941
" date	Feb. 4.	Jan 1.	Jan. 23, '53.
Least mean daily pressure	29·235	29·450	29·086
" " " date	Feb. 4.	Jan 1.	Jan. 23, '53.
Mean daily range	0·251	0·268	0·223
Means at 7 A. M.	29·975	30·082	29·956
" 2 P. M.	29.953	30·056	29·915
" 9 P. M.	29·984	30·082	29·944
" for the year	29·971	30·070	29·938
Force of Vapor—Greatest—inches	0·367	0·361	0 551
" date	Feb. 15.	Dec. 27.	Dec. 2, '59.
Least—inches	047	·038	.013
" date	Feb. 28.	Feb. 23.	Feb. 6, '55.
Means at 7 A. M.	·143	·121	·138
" 2 P. M.	·144	·126	·155
" 9 P. M.	·153	·134	·152
" for the Winter.	·147	·127	·148
Relative Humidity—Greatest—per cent.	96·0	95·0 p. c.	100·0
" date	Feb. 15.	Jan. 21; F.27	Often.
Least—per cent.	29·0	30·0	20·0
" date	Feb. 18.	Jan. 24.	Feb. 22, '64.
Means at 7 A. M.	72·9	79·7	78·2
" 2 P. M.	58·5	65·6	64·9
" 9 P. M.	72·0	78·1	75·3
" for the Winter.	67 8	74·5	72·8
Clouds—Number of clear days*	24·	25·	26·31
" cloudy days	66·	66·	64·2
Means of sky covered at 7 A. M.	64·7 p. c.	65·0 p. c.	62·5 p. c.
" " " 2 P. M.	61·7	60·5	62·2
" " " 9 P. M.	45·7	50·9	48·6
" " " for the Winter.	57·4	58·8	57·8
Rain and melted snow—Amount—inches	11·99	8·94	10·279
No. of days on which rain or snow fell	31·	31·	30·9
Prevailing Winds—Times in 1000	N66°37′W.252	N72°3′W.235	N66°50′W.285

* Sky one-third or less covered at the hours of observation.

// JOURNAL

OF THE

FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE

PROMOTION OF THE MECHANIC ARTS.

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EDITORIAL.

ITEMS AND NOVELTIES.

Bessemer Steel Car Axles.—We subjoin a tabular statement of some very interesting tests lately made by Messrs. Algernon and Percival Roberts, at their establishment, the "Pencoyd Iron Works, near Norristown."

These gentlemen had undertaken the manufacture, for the Pennsylvania Central Railroad, of Bessemer Steel Axles, guaranteed to stand the test of five blows from a ram weighing 1,640 pounds, and falling twenty feet, the axle being reversed after each blow.

To secure this test, each axle is made four feet longer than it is required to be in use, so that a trial piece of this length may be cut off and subjected to the stated number of blows.

Wherever the trial piece fails, the corresponding axle is rejected.

This method of testing is severe and expensive, yet it seems to be the only one which will insure absolute reliability with a mate-

rial like this, where uniformity in the product of manufacture has not yet been secured.

These tables, themselves, need no further explanation, as they speak for themselves, and are so arranged that their various relations are evident on mere inspection.

Trial of transverse strength of Bessemer Steel Car Axles, made at Pencoyd Iron Works. The deflection on the following tests is measured on a chord of four feet.

Diam. of Axle.	No. of Blow.	Weight of Ram.	Height of Fall.	Distance bet. Bearings.	DEFLECTION. Before Blow.	After Blow.	Effect of Blow.
4⅛ in.	1	1640 lbs.	15 ft.	3 ft.	— 00 in.	⌣ 4 in.	4 in.
	2				⌒ 4	— 00	4
	3				— 00	⌣ 3¼	3¼
	4				⌒ 3¼	⌣ ½	3¾
	5				⌒ ½	⌣ 2¾	3¼
	6		25 ft.		⌒ 2¾	⌣ 2¾	5½
	7				⌒ 2¾	⌣ 4½	7¼
	8				⌒ 4½	⌣ 1¼	5¾
	9				⌒ 1¼	⌣ 4¾	6
	10				⌒ 4¾	⌣ 1	5¾
	11				⌒ 1	⌣ 4½	5½
	12				⌒ 4½	⌣ 1½	6
	13				⌒ 1½	⌣ 4¾	6¼
	14				⌒ 4¾	⌣ 1	5¾
	15				⌒ 1	⌣ 4¼	5¼
	16				⌒ 4¼	⌣ 1	5¼
	17				⌒ 1	⌣ 2	3
	18				⌒ 2	Broke,	
					Total	Deflection,	85½ in.

Diam. of Axle.	No. of Blow.	Weight of Ram.	Height of Fall.	Distance bet. Bearings.	Before Blow.	After Blow.	Effect of Blow.
4⅛ in.	1	1640 lbs.	15 ft.	3 ft.	— 00 in.	⌣ 3¾ in.	3¾ in.
	2				⌒ 3¾	— 0	3¾
	3				— 0	⌣ 3½	3½
	4				⌒ 3½	— 0	3½
	5				— 0	⌣ 4	4
	6		25 ft.		⌒ 4	⌣ 2½	6½
	7				⌒ 2½	⌣ 2¼	4¾
	8				⌒ 2¼	⌣ 2½	4¾
	9				⌒ 2½	⌣ 3½	6
	10				⌒ 3½	⌣ 2	5½
	11				⌒ 2	⌣ 3½	5½
	12				⌒ 3½	⌣ 1¾	5¼
	13				⌒ 1¾	⌣ 3½	5¼
	14				⌒ 3½	⌣ 1¾	5¼
	15				⌒ 1¾	⌣ 3¼	5
	16				⌒ 3¼	⌣ 2	5¼
	17				⌒ 2	⌣ 3	5
	18				⌒ 3	⌣ 2½	5½
	19				⌒ 2½	⌣ 3	5½
	20				⌒ 3	⌣ 1¼	4¼
	21				⌒ 1¼	⌣ 3	4¼
	22				⌒ 3	⌣ 1¼	4¼
	23				⌒ 1¼	Broke.	
					Total	Deflection,	106¼ in.

4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 00 in.	⌣ 5½ in.	5½ in.
	2				⌢ 5½	⌢ ½	5
	3				⌣ ½	⌣ 5½	5
	4				⌢ 5½	⌣ 1	6½
	5				⌢ 1	⌣ 4¾	5¾
Trial discontinued. Up to standard required by Penn'a R. R.							
4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 00 in.	⌣ 5½ in.	5¼ in.
	2				⌢ 5¼	— 00	5¼
	3				— 0	⌣ 5	5
	4				⌢ 5	⌢ ½	4½
	5				⌢ ½	⌣ 4½	4
Trial discontinued. Up to standard required by Penn'a R. R.							
4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 6 in.	6 in.
	2				⌢ 6	⌢ 1½	4½
	3				⌣ 1½	⌣ 6	4½
	4				⌢ 6	⌢ 2¼	3¾
	5				⌣ 2¼	⌣ 6¼	4
Trial discontinued. Up to standard required by Penn'a R. R.							
4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5 in.	5 in.
	2				⌢ 5	— 0	5
	3				— 0	⌣ 2½	2½
	4				⌢ 2½	⌣ 1½	4
	5				⌢ 1½	⌣ 2⅝	4⅛
Trial discontinued. Up to standard required by Penn'a R. R.							
4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5¼ in.	5¼ in.
	2				⌢ 5¼	Broke.	
4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌢ 5 in.	5 in.
	2				⌢ 5	Broke.	
4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5¾ in.	5¾ in.
	2				⌢ 5¾	⌢ ½	5¼
	3				⌣ ½	⌣ 5½	5
	4				⌢ 5½	⌢ 1½	4
	5				⌣ 1½	⌣ 5½	4
Trial discontinued. Up to standard required by Penn'a R. R.							
4⅛	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌢ 4 in.	4 in.
	2				⌢ 4	Broke.	
4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 4¾ in.	4¾ in.
	2				⌢ 4¾	⌣ 1	5¾
	3				⌢ 1	⌣ 3¾	4¾
	4				⌢ 3¾	⌣ ¾	4½
	5				⌢ ¾	⌣ 2½	3¼
Trial discontinued. Up to standard required by Penn'a R. R.							

$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌒ 5 in.	5 in.
	2				⌒ 5	⌒ 1	6
	3				⌒ 1	⌒ 3	4
	4				⌒ 3	Broke.	
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	‿ 5 in.	5 in.
	2				⌒ 5	— 0	5
	3				— 0	‿ $4\frac{1}{2}$	$4\frac{1}{2}$
	4				⌒ $4\frac{1}{2}$	— 0	$4\frac{1}{2}$
	5				— 0	‿ $4\frac{1}{4}$	$4\frac{1}{4}$
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	‿ 5 in.	5 in.
	2				⌒ 5	⌒ $\frac{1}{4}$	$4\frac{1}{2}$
	3				‿ $\frac{1}{2}$	‿ 5	$4\frac{1}{2}$
	4				⌒ 5	⌒ $\frac{1}{2}$	$4\frac{1}{2}$
	5				‿ $\frac{1}{2}$	‿ $4\frac{1}{8}$	$4\frac{3}{8}$
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌒ $5\frac{1}{2}$ in.	$5\frac{1}{2}$ in.
	2				⌒ $5\frac{3}{4}$	Broke	
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	‿ $4\frac{3}{4}$ in.	$4\frac{3}{4}$ in.
	2				⌒ $4\frac{3}{4}$	— 0	$4\frac{3}{4}$
	3				— 0	‿ $4\frac{1}{4}$	$4\frac{1}{4}$
	4				⌒ $4\frac{1}{4}$	⌒ $\frac{1}{2}$	$3\frac{3}{4}$
	5				‿ $\frac{1}{2}$	‿ $4\frac{1}{2}$	4
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	‿ 5 in.	5 in.
	2				⌒ 5	— 0	5
	3				— 0	‿ 5	5
	4				⌒ 5	⌒ $\frac{1}{2}$	$4\frac{1}{2}$
	5				‿ $\frac{1}{2}$	‿ 5	$4\frac{1}{2}$
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0	‿ 5	5
	2				⌒ 5	— 0	5
	3				— 0	‿ $4\frac{3}{4}$	$4\frac{3}{4}$
	4				⌒ $4\frac{3}{4}$	— 0	$4\frac{3}{4}$
	5				— 0	‿ $4\frac{1}{2}$	$4\frac{1}{2}$
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	‿ $4\frac{3}{4}$in.	$4\frac{3}{4}$
	2				⌒ $4\frac{3}{4}$	— 0	$4\frac{3}{4}$
	3				— 0	‿ $4\frac{1}{2}$	$4\frac{1}{2}$
	4				⌒ $4\frac{1}{2}$	⌒ 1	$3\frac{1}{2}$
	5				‿ 1	‿ $4\frac{3}{4}$	$3\frac{3}{4}$
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	‿ $4\frac{3}{4}$ in.	$4\frac{3}{4}$ in.
	2				⌒ $4\frac{3}{4}$	⌒ $\frac{1}{2}$	$4\frac{1}{4}$
	3				‿ $\frac{1}{2}$	‿ 4	$3\frac{1}{2}$
	4				⌒ 4	Broke.	

4⅛ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 6⅛ in.	6⅛ in.
	2				⌢ 6⅛	⌢ ½	5⅝
	3				⌣ ½	⌣ 6	5½
	4				⌢ 6	Broke.	
4⅛ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5¼ in.	5¼ in.
	2				⌢ 5¼	Broke.	
4⅛ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 6 in.	6 in.
	2				⌢ 6	— 0	6
	3				— 0	⌣ 6	6
	4				⌢ 6	— 0	6
	5				— 0	⌣ 6	6
Trial discontinued. Up to standard required by Penn'a R. R.							
4⅛ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 6 in.	6 in.
	2				⌢ 6	Broke.	
4⅛ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	Broke.	
4⅛ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣6¼ in.	6¼ in.
	2				⌢ 6¼	—0	6¼
	3				— 0	⌣6	6
	4				⌢ 6	Broke.	
4⅛ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 6½ in.	6½ in.
	2				⌢ 6½	— 0	6½
	3				— 0	Broke.	

CELESTIAL CHEMISTRY.—This is a department of science now recognized as a distinct and (by reason of the large amount and wonderful character of the knowledge which it already includes) an important branch of modern research; and we propose at the present time to group together under the above general heading, a number of interesting results obtained within the last year, and in some cases briefly noted by us before at the time of their first announcement, but of which full accounts have now for the first time reached us, chiefly in a large package of the Proceedings of the Royal Society, which has just arrived. Taking these in the order of their novelty, as far as may be consistent with a consistent classification of subjects, we will begin with—

The Spectrum of Comet II of 1868, and its coincidence with that of ignited carbon vapor. In the Proceedings of the Royal Society, p. 481, Mr. Wm. Huggins gives an account of his observations and experiments with regard to this object. The comet as

seen in the telescope has the appearance as shown in the accompanying cut, Fig. 1, consisting of a nearly circular coma, becoming

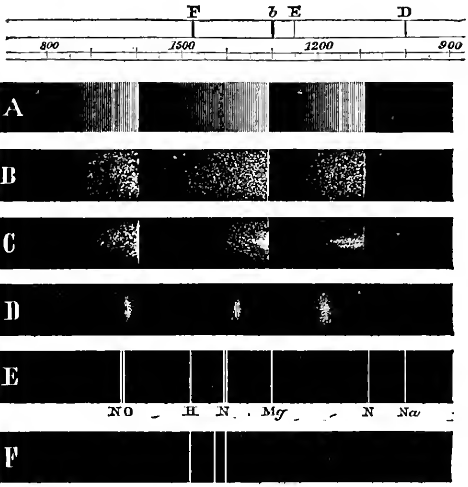
Fig. 1.

suddenly bright towards the centre, which consisted of a nearly round spot of light. The tail, of a hirsute character, extended nearly one degree (about twice the apparent diameter of the moon).

When the light from the circular head was examined with a spectroscope having two prisms of 60°, three bright bands were observed such as are shown in C of Fig. 2.

The two of these bands towards the left faded off towards that side by a regular gradation, but the other did not show this character. These bands showed no indication of divisibility into lines, nor did any of them shade off towards the left or violet end of the spectrum. On comparing these bands with those of other spectra, it was found that they coincided exactly with those given by ignited carbon vapor under certain conditions. Thus in 1864, Mr. Huggins had observed that when the spark from an induction coil with leyden jars in circuit, was taken in a current of olefiant gas, the spectrum shown in B, Fig. 2 was obtained. This coincided with the carbon spectrum obtained under other conditions, except that no separate, strong lines were to be distinguished, and in place of the shading composed of fine lines, found in what might be called the normal carbon spectrum, (see Fig. 2,

Fig. 2.



A, showing the spectrum of the spark in olive oil,) an irresolvable nebulous light was here given. It should be here remarked, that in the actual experiment, the lines due to the other bodies, such as Hydrogen and Platinum, were also seen, but these being well known, were readily eliminated. The drawing shows the lines due to carbon alone. On the 23d of June, 1868, the spectrum of the comet was compared directly with the spectrum of the spark in olefiant gas, and the identity of position and character between the bands was then very manifest, as appears in Fig. 2, B and C.

We are thus led to the strange conclusion that this comet is composed of luminous carbon vapor. Now we know, that to vaporize carbon, as we have it within the range of experiment, requires heat of great intensity and peculiar conditions of chemical reaction, as in the case of a jet of olefiant gas supplied with air, which gives a a greenish flame, resolvable into the carbon spectrum.* In this connection we would refer to the paper on "Sources of Light in luminous Flames," at page 330 of this number.

How any such conditions of temperature as those developed by the induction spark which produces carbon vapor directly, or those of the mixed air and gas flame where it is indirectly developed, can subsist in a comet, we are utterly at a loss to imagine. This mystery, however, does not stand alone, for in the case of the gaseous nebulæ we have a like example of self-luminous gas maintaining its luminosity with a similar absence of adequate cause. It would appear possible that some action related to phosphorescence or to fluorescence might be concerned in this result, but such a conjecture is unsupported by any evidence of similarity between the spectra of such bodies and of these celestial objects. We can then simply regard this comet as a vast globe of glowing carbon vapor, having a density approaching that of our atmosphere, (for it is at atmospheric pressure that the spectrum B, Fig. 2, is given,) maintained in its luminous state by forces and conditions of which we have absolutely no knowledge.

In connection with this subject, Mr. Huggins suggests that the enormous velocity with which the tails of comets are repelled from the sun, may be connected with a previous dilation of substance by means of which the particles have been separated beyond the range of cohesive attraction.

* We have just observed that the same effect is obtained with a Bunsen burner, supplied with common gas, by observing the lower part of the flame.

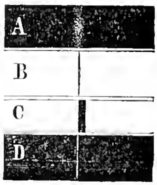
Spectrum of Brossen's Comet.—A notice of this spectrum at the time of its first observation was published in this *Journal*, Vol. LVI, p. 155, but we now give, in D of Fig. 2, a drawing of its bands, which may be compared with the bright lines of certain elements given with the induction spark (see E), and with the lines of the nebulæ, such as 37 H, IV Draconis, 1 H, IV Aquarius, the great nebulæ in Orion, and several others. It is evident at a glance that this spectrum differs essentially from that of the other comet, but no conclusion seems as yet to have been reached as to its probable constitution. The division of the middle band into two bright lines is a curious feature, worthy of note.

Physical Constitution of the Sun, as revealed by the Spectroscope.—In a preliminary note concerning observations and experiments in this direction, which they have now in progress, Prof. Frankland and Mr. Lockyer briefly notice several interesting points. Reference is first made to the paper by Mr. Lockyer, of which we republished the abstract in our last number (p. 266), and in which attention is directed to the fact that, (1) there is a continuous gaseous envelope about the sun, called the "chromosphere," and that the H β line, (corresponding to Fraunhofer's F,) in the spectrum of this envelope, widens out as it approaches the body of the sun, so as to have a lance-head shape. (2). Ordinarily in a luminous prominence, the line H β is nearly of the same width as H α, (corresponding with C), but that sometimes H β widens out so as to present a bulbous appearance above the chromosphere. See Fig. 5, plate facing p. 268, last number. (3). The lines H α and H β extend over the photosphere and reverse the corresponding Fraunhofer lines. (4). There is no Fraunhofer line corresponding to the one near D in the spectrum of the chromosphere. (5). There are many *bright* lines visible in the light of the photosphere near the edge. (6). A new line occasionally appears in the spectrum of the chromosphere.

In following out the investigations indicated by these observations, they have, in the first place, been unable to develop in the spectrum of hydrogen any line corresponding with that near D, in the spectrum of the chromosphere. Secondly, they have found that the expansion of the lines in the spectrum of hydrogen, already described by Plucker and Hittorf, is due to the *pressure* in the gaseous medium, and not at all to temperature *per se.* Thus, in the researches of these German savants, quoted by us on p. 272 of our last number, it was mentioned that when the spark length was

increased, the broadening of the lines occurred. Now, we know that this increase of spark length implies an increase of temperature under the conditions involved in these experiments, but it also must increase (in the closed tubes which were employed to contain the gases) the tension or pressure on these expansible fluids. The previous experiments, therefore, did not indicate to which of these influences the expansion of the lines was due. This has, however, been accomplished by Messrs. Lockyer and Frankland, and though we are not yet informed as to the methods pursued, these are not difficult to imagine. In this connection it is curious to note that the spectrum of the star Sirius, observed by Mr. Huggins with the instrument of great dispersive power, prepared for the measurements of advance or retreat of stars, with reference to the earth (noticed at page 155 of our last Volume), shows an expanded hydrogen band, F, much broader than that found in the solar spectrum, and corresponding in fact with that obtained from the spark in hydrogen at a pressure approaching that of the atmosphere. These relations are illustrated in Fig. 3, where A represents the F line, as given at this partial atmospheric pressure, D the same in an exhausted tube, B the F line of absorption in the solar spectrum, and C that in the spectrum of Sirius.

Fig. 3.



Now, it has long been surmised that the mass of Sirius is far greater than that of the sun, (393 times as great, according to the usual estimate,) and thus a greater density of the heated hydrogen at its surface, would be exactly what we might expect to find.

In our next number (want of space now precludes us), we will give a full account of the very curious investigations as to the proper motion of the stars above noticed, and of which we have now at hand full details.

To return to the "Note" of Messrs. Frankland and Lockyer; they further remark, that their experiments have shown the hydrogen gas contained in the solar prominences to be in a condition of extreme tenuity, the bulbous appearance of the F line indicating local currents or intensities of temperature.

Thirdly, from the absence of any black line in the solar spectrum corresponding with the bright one near D given by the chromosphere, implies, that if this is a hydrogen line, the absorption of the atmosphere is unable to reverse it. In explanation of this, it must

be remembered that the light which gives us the spectrum of the chromosphere comes from a stratum of great thickness, measured in the direction of the line of sight (nearly 200,000 miles), while the thickness of the chromospheric shell itself, which alone is effective in absorbing and reversing the rays, is small in comparison (5000 miles). Thus the one might produce a luminous line of appreciable force, while the other would not absorb enough light to develop a sensibly dark space in the solar spectrum.

Fourthly, it would appear that the photosphere is no longer of necessity to be considered as composed of incandescent solid or liquid matter, but may be of gaseous consistency, this being consistent with its continuous spectrum. (This follows, in fact, from the observations of Plucker).

Fifthly, the remarkable fact is next stated in this preliminary note that: *Bright lines have been discovered in the spectrum of the photosphere*, in the vicinity of the solar edge. The evidence of such lines had been surmised by Prof. Stoney, and in a communication to the Royal Society, May 15, 1867, he alludes to the probable position of several.

Tidal Rains.—In the *Journal* for November, 1868 (p. 304), we referred to Mr. P. E. Chase's investigations of tidal influences on the fall of rain. In the present number, we give some of his curves, which seem to furnish conclusive evidence of the following tendencies:—

1. A diminution, both in the frequency and in the amount of rain, near the times of new and full moon, with an increase in the subsequent octants.

2. A resemblance between the lunar-daily and the solar-hourly barometric changes. "There are, however, only two normal barometric maxima and two minima during the solar day, while there are three of each during the lunar month. When the moon's upper culmination occurs at night, she intensifies for the whole day, the barometric tendency of the corresponding solar hour; when it occurs by day, this intensification is accompanied and controlled by a marked priming and lagging which introduce an additional inflection into the lunar curve, the normal barometric tendencies being accelerated before, and retarded, after new moon."

3. Triple periods of increase and diminution in the lunar-hourly and the lunar daily rains, nearly corresponding in period and opposed in direction to those of the barometer.

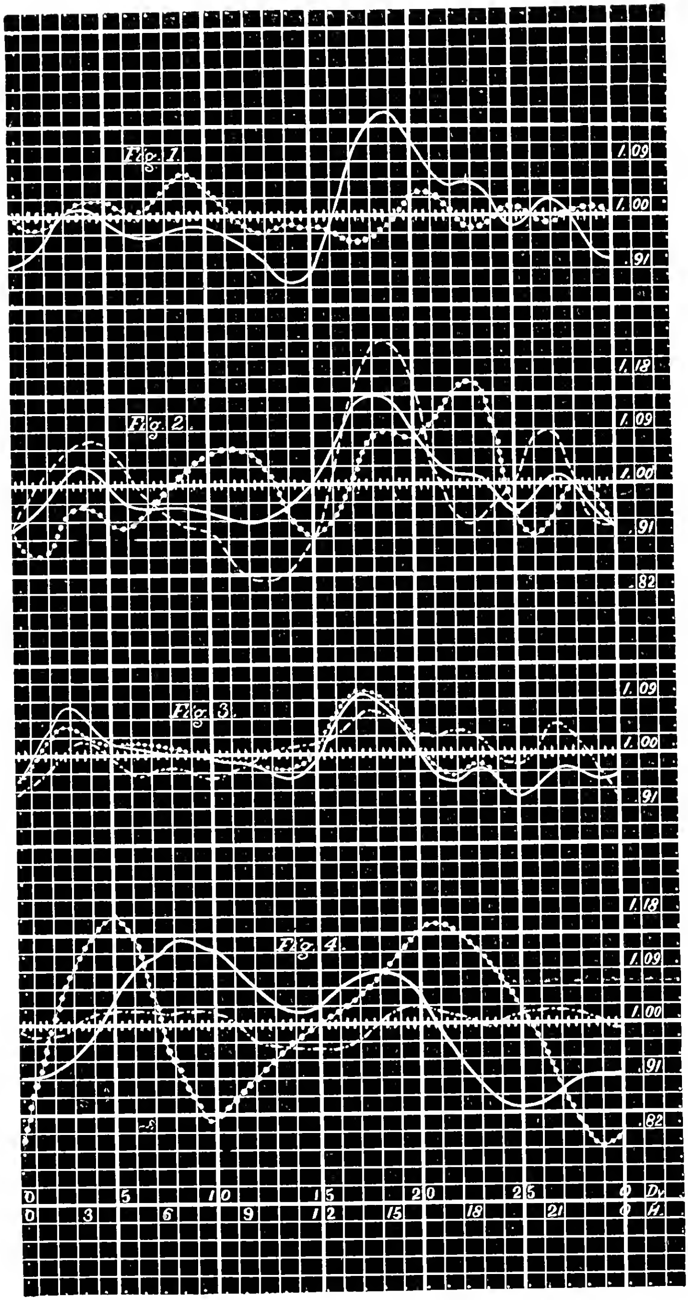


Fig. 1.
1.09
1.00
.91
1.18
Fig. 2.
1.09
1.00
.91
.82
1.09
Fig. 3.
1.00
.91
1.18
Fig. 4.
1.09
1.00
.91
.82
0 5 10 15 20 25 0 Dy.
0 3 6 9 12 15 18 21 0 H.

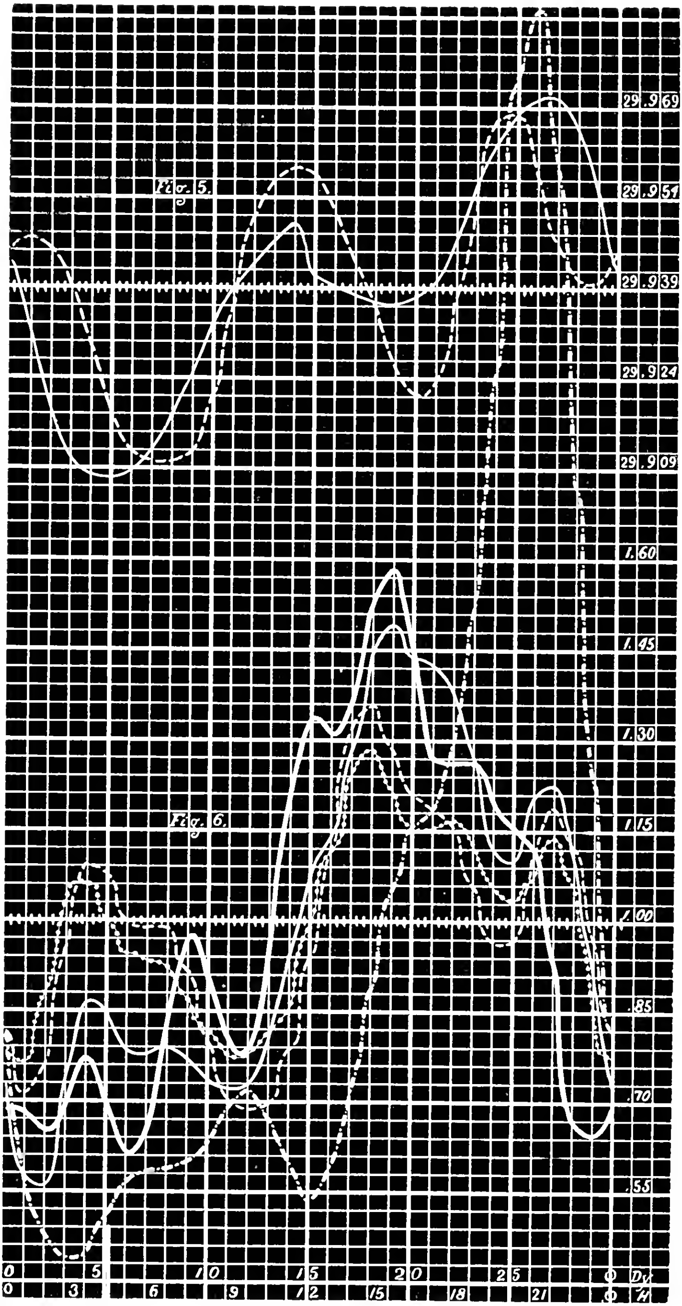


Fig. 5.
29.969
29.951
29.939
29.924
29.909
1.60
1.45
1.30
Fig. 6.
1.15
1.00
.85
.70
.55
0
5
10
15
20
25
0
Dy
0
3
6
9
12
15
18
21
0
H

4. Heavy rains after full moon, "or during that portion of the lunar month when the moon's action is intensified by a falling barometric and increasing condensation of atmospheric vapor."

At the recent meeting of the National Academy of Sciences, in Washington, Mr. Chase exhibited some of the curves which he had computed for the Coast Survey. They illustrated several interesting facts, among which were the following:—

1. Opposite tendencies at the equinoxes and solstices.
2. Opposite tendencies at Greenwich and Philadelphia, or on opposite sides of the ocean.
3. The influence of mountain ranges on the times, as well as on the amounts of precipitation.
4. Planetary influence.
5. Cycles of dry and rainy years, dependent on the relative positions of the moon and principal planets.

EXPLANATION OF FIGURES.

The notched horizontal line, in each figure, represents the mean daily or hourly value; each vertical space represents a deviation of ·03 of the mean value, in the rain curves, or of ·003 of an inch in the barometric curves; each horizontal space represents a day in the abscissas of the monthly curves, or forty-eight minutes in the abscissas of the daily curves.

Fig. 1. Total lunar-daily rainfall.
Philadelphia, 1825–44; ———————continuous line.
Surrey, 1825–44; .... dotted line.

Fig. 2. Total lunar-daily rainfall at Philadelphia.
1825–44; .... dotted line.
1845–64; - - - - - - - - - - broken line.
1825–68; ——————— continuous line.

Fig. 3. Lunar daily rains at Philadelphia.
No. of storms exceeding ·25 in.; .... dotted line.
Average fall; - broken line.
Amount of rain in moderate storms; ——————continuous line.

Fig. 4. Total hourly rainfall.
Greenwich, solar hourly; .... dotted line.
Philadelphia; " " ——————— continuous line.
" lunar hourly; - - - - - - - - - - broken line.

Fig. 5. Barometric fluctuations at Philadelphia.
Solar hourly; ———————— continuous line.
Lunar daily; - - - - - - - - - - - - broken line.

Fig. 6. Heavy rains at Philadelphia.
Lunar daily amounts, in storms of 1 inch or more; dotted line.

Do.	do.	do.	1·5 in. or more; broken line.
Do.	do.	do.	2 in. or more; light line.
Do.	do-	do.	2·5 in. or more; heavy line.

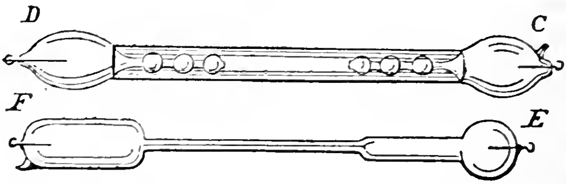
Solar-hourly, exceeding 25 in. per hour; dotted broken line.

Invisible Light.—Many years since, a photograph was made at Berlin of the well known bronze statue of the Amazon, and it was observed that in the negative a black streak occurred at the tip of the lance (held by the figure in an almost vertical position), while two other analogous marks appeared in other locations. This picture was sent to Professor Dove, whose investigations in connection with light are widely known, and he came to the conclusion that these markings might be due to electrical discharges going on from prominent points of the figure at the time the picture was taken, and which, though invisible to an observer, would nevertheless, by reason of the high actinic power of electric light, produce an impression on the photographic plate.

This conjecture was fully confirmed by Professor O. A. Rood, of Columbia College, N. Y., who, in a series of ingenious experiments, proved, that electric discharges entirely invisible to the observer in the presence of daylight, might nevertheless produce images of themselves in a picture of the adjacent objects taken at the same time—the photographic plate being relatively more sensitive to these impressions than the human eye.

Among other means used to demonstrate this fact, a number of Geissler tubes were employed. These, as our readers know, are glass tubes of various shapes, such as the accompanying cuts illus-

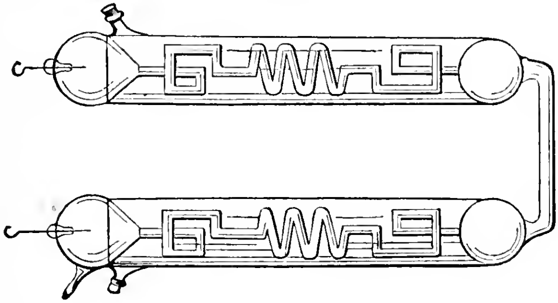
Fig. 1.



trate, very perfectly, though not absolutely exhausted, and sealed up, but with platinum wires secured in their extremities. When

the electric discharge from an Induction Coil was made to pass through these, it developed a delicate purple and blue, or sometimes pink light, which, though visible in the dark, was not perceptible in daylight. A photograph, however, of these tubes, taken in daylight, showed as well developed an image of the interior light, or

Fig. 2.



discharge, as of the tube itself. If, however, a "fluorescent"* medium were placed around the tube carrying the discharge (as when the jackets in such a tube as Fig. 2 were filled with a solution of quinine), a bright light was visible, but no impression was produced upon the sensitive plate. The practically invisible actinic rays were here made visible, but lost in photographic force what they gained in optical power. We have thus far cited examples of invisible light, which, though properly indicated by the title used, were still not perfect in their invisible character, since, under a new state of the surrounding conditions they might cease to possess the characteristic of invisibility.

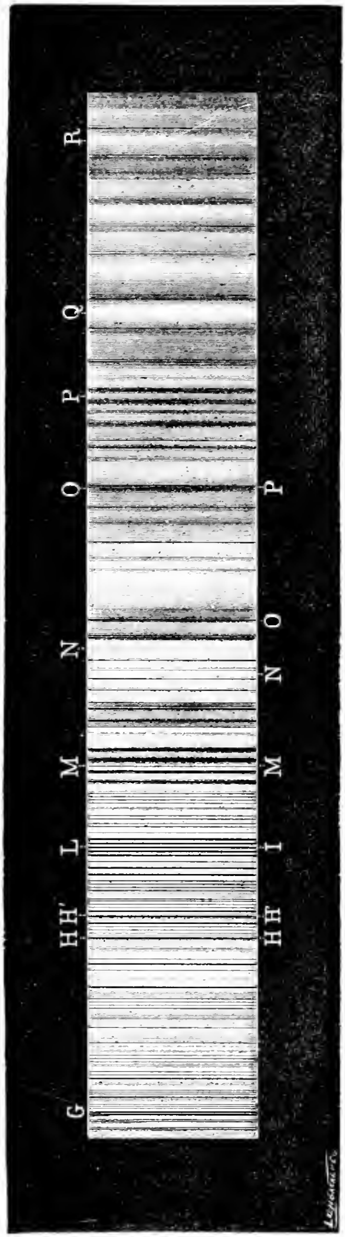
We have, however, other illustrations of the same paradoxical condition which are free from this objection.

If a spectrum is produced with sunlight, by means of prisms and lenses of quartz, we shall see all the beautiful varieties of pure blending colors known as the rainbow tints, crossed by the black lines, named after Fraunhofer, exactly as with the prisms and lenses of glass, but if we allow this "spectrum" image to fall upon a collodion plate, the picture produced (besides showing the visible bands in the blue, indigo and violet parts of the spectrum), will give an equally distinct drawing of similar bands or lines, reaching

* Fluorescent bodies are those which have the power of absorbing very rapid vibrations, and re-emitting them in a condition of slower motion; taking up, for example, actinic and invisible rays and emitting them as luminous ones.

to a great distance beyond, what, to the eye, is the end of the luminous stripe. Fig. 3 shows the important part of such a picture. It is copied from an original photograph mentioned in this Journal, Vol. LVI, p. 303, made by Prof. Muller. The line G is one of those known and named by Fraunhofer, and occurring in the indigo portion of the spectrum; H and H′ were also named by him, and occupy the extreme violet. Near these the visible spectrum terminates, and all beyond is purely actinic force, which cannot be appreciated by the eye directly, under any conditions. This part of the picture (all beyond H′, in other words,) may be regarded as a photograph not of *light*, but of *darkness*. Not, however, of *any* or *all* darkness, but of a certain kind of darkness only. The letters on the cut above the spectrum are those assigned as names to the important groups of lines by M. Mascart, who has written a work on the extra-violet spectrum; while the letters below are those assigned by M. Becquerel, to whom this department of science owes very much. This portion of the spectrum may be made appreciable to the eye *indirectly*, if it is allowed to fall on a screen made of some fluorescent substance, when, as in the case of the Geissler tubes, before mentioned, the motions or vibrations too rapid to affect the eye, will be so diminished in their rate as to develop some appreciable color varying with the fluorescent substance employed.

Fig. 3.



Civil and Mechanical Engineering.

GAUGING THE FLOW OF RIVERS, &C.

By Clemens Herschel, C. E.

On a means of measuring the quantity of water flowing in rivers, canals, etc., by the method of first measuring the *velocity* of this flow at a number of *points* in the cross section of the stream.

In the Lowell Hydraulic Experiments of James B. Francis, we have the record of a series of experiments on the flow of water over weirs of a certain construction, such flow during a certain time being gauged by direct measurement after entering a receiving basin of known and regular dimensions.

In the second edition of the same work, we find, further, the record of a series of experiments on gauging the flow of water in rectangular canals or flumes, by means of loaded tubes or poles, which measurements were corrected by gauging the same body of water as it passed over a weir, by the previously established reliable method or formula. We can, therefore, not only take this work to be a *standard* authority on these two methods of gauging the flow of water, but we can also, without I think exaggerating the value of the book, call it a standard authority on these two subjects for all time.

There arise, however, many cases in which neither of the above two methods are practically or conveniently applicable, and it is to meet these, that the method indicated in the heading, is of great service. Such cases are, in the first place, all natural streams, large and small rivers, brooks, etc., that have no dams across them at the place of investigation, also rough canals, and even well constructed rectangular flumes, provided they are not long enough to admit of a good measurement with tubes, and generally, when as so frequently happens, a weir is inapplicable on account of the back-water it causes, and a long and properly situated rectangular flume cannot be put in, either on account of want of room, or of time, or because the same would cost more than the parties interested are willing to expend.

In these cases, if we find the cross section of the stream on any given line, divide it into any chosen number of parts, measure the

velocity of the current at the centre of these parts, and multiply their area by these measured velocities, we get, of course, in the sum of these products, the total quantity flowing through the whole cross section.

Practically considered, the accuracy of such a measurement will depend *first*, on making the partial areas sufficiently small, which implies the measuring of the velocity in a sufficiently great number of points; *secondly*, on the accuracy of the instrument used to observe the velocity of the current, and on the accuracy with which it is used, and *thirdly*, and finally, on introducing a coefficient, to be determined by experiment—just as the weir and tube formulæ were found and corrected by experiments—because we cannot measure the velocity of those water lines which are in immediate contact with the periphery, as will appear presently. It may be well to state at once that this coefficient is small, and I doubt if on trial, provided the experiments are conducted with the accuracy of those described in the Lowell Hydraulic Experiments, it will be found to affect the total result in the case of canals as much or more than 3 per cent., and perhaps no more in the gauging of rivers of any size. Hitherto, this coefficient has never been considered at all. The best instrument to measure the velocity of the current at any point, has generally been thought to be the so-called Woltman wheel, or tachometer (Woltmanscher Flügel Ger., moulinet de Woltman Fr.) descriptions of which may be found in Weisbach's Mech., Vol. II., Bauernfeind Vermessungs kunde, Annales des P. & C., Nov. and Dec. 1847, etc.

It is perhaps unnecessary to repeat these here; any interested reader of this, will look them up, provided the subject is not already familiar to him, and those that do not care to do this, will have to get along the best they can.

Let us assume, therefore, that this little instrument, invented as far back as 1780 or '90, is well known to us all, and proceed.

To simplify matters, and at the same time to introduce in the English language an easy sounding name for the instrument, we will call every instrument that measures the velocity of a current, by the number of revolutions imparted by this current in a certain time, to a body revolving on an axis of whatever shape, a "moulinet" (French name for them, signifying little mill), and a gauging made by using such an instrument, a "moulinet measurement."

There have been two chief objections to the use of Woltman's

moulinet, as taught in the various text books and articles; one was the trouble of being obliged to lift the instrument and the rod to which it was attached, out of the water at every reading; the other was the trouble of first determining by experiment what that reading meant, when expressed in velocity of feet per second. There may be added the danger of injury to the moulinet, but as will be shown, this danger can be done away with for the greater part, by a more rational arrangement of the whole apparatus.

As regards now the first named evil, a very serious one, as it wastes at least half the available time, and also exposes the moulinet to great danger of injury, it is a pleasure to be able to record, that it has been entirely and very neatly eradicated by an invention of Mr. D. Farrand Henry, Assistant of the U. S. Lake Survey, which has been employed in the gauging of the rivers connecting (or leading to the sea), the Great Lakes, under the charge of General W. F. Raynolds, U. S. Eng., and stood the test of service. It consists, in the abstract, of the idea of connecting the revolving float or propeller wheel, etc., by means of an electric circuit with a register above water, so that each revolution is recorded as it is made.

The Figs. 1—5, show two of the telegraphic moulinets employed, and the manner of their use. Fig. 1 shows the cups and wheel of a Robinson anemometer,* as they were arranged and used as a telegraphic moulinet.

On the axis is a small arm, which comes in contact with a fine silver wire at every revolution. This wire, in order to give it more elasticity, is formed into a spiral, then is insulated from the frame, and finally connected with one of the battery wires; the other is connected with the frame in which the wheel is hung. This frame

* This anemometer takes its name from that of its inventor, Dr. Robinson, of Armaugh, Scotland. Its construction is based upon Dubuat's observations, that the resistance of a sphere floating in water is to that of its great circle as 0·35 : 1. (See Principes d' hydraulique Vol. II., p. 263.) Beaufoy found this relation to be 0·342, and hence Robinson supposes (without just cause it seems to me), that a four armed wheel having four hemispherical cups at its periphery (see Figs. 1 and 5), would revolve with a velocity of revolution, at the centres of the cups, equal to one-third of that of the current of air to which it was exposed. The anemometer wheels used, gave very good results in *water*, inasmuch as they registered velocities as low as 0·35 feet per second, but the "coefficient" of their revolution varied with the velocity observed, being more and less than one-third, as may be seen hereafter. The anemometer wheels here spoken of were made by James Green, of New York.

is supported in a second frame, behind which latter is fastened a vane to keep the axis in the line of the current, as shown in Fig. 5.*

Fig. 2 shows another kind of moulinet, in which the wheel is a kind of propeller. In this one, an eccentric is placed upon the hub of the propeller, and a little roller at the end of the ivory lever is pressed against it by means of an adjustable spring. On the under side of this lever, projects a small piece of platinum wire, and this wire comes in contact with a platinum plate just below it, whenever the eccentric is at its minimum. This platinum wire is connected by means of an insulated wire, with one pole of the battery, and the axis of the propeller with the other. This propeller also is hung in two frames, and is furnished with a vane similar to the first mentioned.†

Figs. 3 and 4 show the register in front and rear elevation. It is a very simple contrivance, and consists of a common telegraphic sounder, which moves the first cog wheel forward one cog, at every movement of the lever, by means of a pawl and ratchet. This wheel gears with a ten-leaved pinion into the second hundred toothed cog wheel, and we have thus a register up to 1000 movements of the sounder. Two hands serve to indicate the number of these movements on two dials. The mode of operation is shown in Fig. 5, (not drawn to scale.) A boat in this case, 15—16 feet long, and about three feet beam, is furnished with an anchor, for a current of about six feet per second, to weigh forty pounds, and a dead weight of the same weight. At the fluke end of the anchor-stock, is fastened a line about 200 feet long, and another line is made fast to the head ring. The boat is now rowed out into the stream to a point about 200 feet above the place of observation, the anchor is thrown out and the boat is allowed to drop down with the current until the line attached to the fluke-end has nearly run out. This line is now made fast to the forward ring of the dead weight, and a second line and a strong copper wire are made fast to the upper ring. The dead weight is now lowered, and the boat is at the same time permitted to drop down stream until finally, when the weight is at the bottom, it is vertically under the stern of the boat. The forward line is now made fast, and the line fastened to the upper ring of the

* The drawings are not to scale, but the circumference of the circle described by the centres of the cups is given = 3·52 feet, whence and from the following tables, the velocity of revolution, etc., may be calculated.

† Of course any other kind of wheel, such as that of Woltman's, for example, may be fitted in a similar manner to register by telegraph.

Fig. 3.

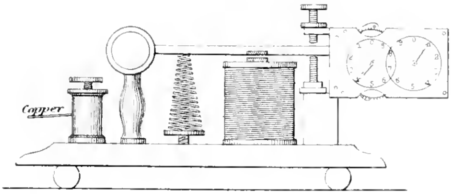


Fig. 1.

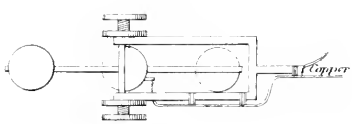


Fig. 4.

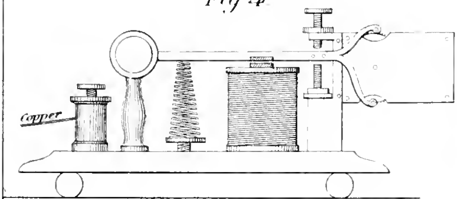


Fig. 2.

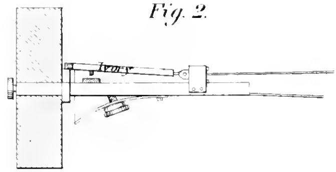
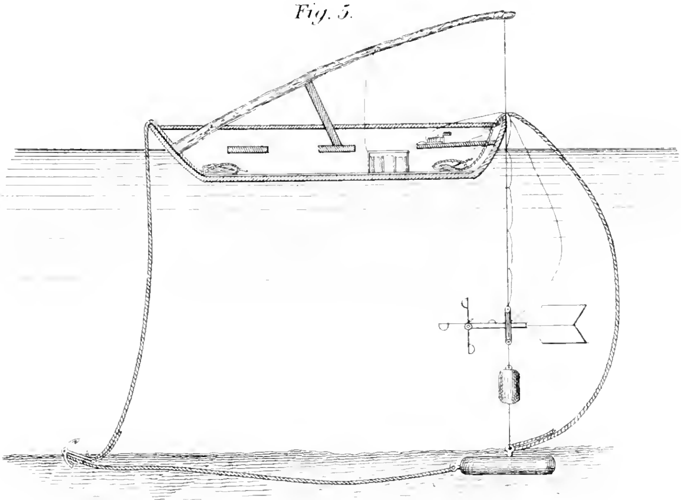


Fig. 5.



weight is let out slack, so as to be out of the way, and is then made fast astern.

As may be seen in the figure, there is a spring-pole over the boat. The after end of this pole is bent down, and the copper-standing wire is fastened to it. This arrangement serves to keep the standing wire taut, and to take up small wave movements of the boat. The outer frame of the moulinet has a swivel ring at the top and bottom; a weight is attached to the lower, and a measured cord, having small snap hooks every five feet, to the upper one. These hooks are snapped around the standing wire, thus keeping the measured cord as straight as may be, instead of being bowed out with the current.

On one side of this same frame are two eyes, through which is placed the standing wire, and along which wire, the whole apparatus may now be raised or lowered without disturbing the electric current in its passage from the frame through the eyes and the standing wire to the battery. The moulinet is now lowered to a depth of say five feet, and the ends of the standing and insulated wires are connected with the battery and register in the usual manner. A common electric switch serves to put the register in and out of connection, and this enables us to observe the number of revolutions made by the moulinet wheel in any desired length of time. The moulinet may now be lowered to any other desired depth by means of the measured cord, the position of the boat being determined at each place of observation by measurements from the shore. When all desired moulinet observations have been made at one place, the instrument is hauled up and detached from the standing wire, the upper end of the same is let loose, the dead weight is hauled up on the upper ring and line, and the anchor may then be easily raised, even from a clay bottom by means of the connecting line, which pulls directly on the flukes. The above account is reproduced from data furnished by Mr. Henry, and although the full results of his observations are not yet ready for publication, the following notes may be of interest. Observations were taken on a fixed line every 100 feet width, and every five feet depth. It was at once found—and a Woltman moulinet when running near the surface shows the same thing—that the water had no *uniform* motion, but *pulsated*, so to speak. These pulsations again were not regular, but had a common maximum every minute and a half, and a still greater maximum or a minimum every five or ten minutes. These pulsations were weakest in the main current, and largest near

the shores and bottom. A table of the discharge of the five rivers that connect and bring to the ocean the waters of the Great Lakes, was published in the number of the *Journal Franklin Institute* for January, 1869.

Careful experiments have also been made to compare the velocities as indicated by submerged floats—such as were used by Generals Humphreys and Abbott—with those given by the telegraphic moulinet, from which it appears that the former show a velocity slightly in excess of the true one, caused by the upper float dragging the submerged one with it.

It will be remembered that in all moulinets, the so-called "coefficient" of the instrument (=the velocity of the current in feet per second divided by the number of turns per second), has played an important part, and although, as will presently appear, I believe this troublesome member may be entirely abolished, yet it is only just to insert here, Mr. Henry's studies on the same, the more so as they were conducted with great care and accuracy. Mr. Henry also claims to have discovered that the coefficient of *all moulinets* varies with the velocity measured and according to a given ellipse, or a part of the same, in which the abscissæ represent the velocities, and the ordinates the coefficients. The coefficients of the two moulinets here drawn, were found by fastening them under the middle of a boat, which was then pulled by a rope with various velocities through a lake, and over a course of about 500 feet.

The following Table I. gives the results of these experiments, and it is to be noted that the given figures are the mean values of 20—30 observations in case of the slow velocities, and 8—15 observations for the others.

These coefficients appear to follow some law of increase, and Mr. Henry finds that they approximate nearest to forming the ordinates of a quarter ellipse, whose semi-major axis, representing the velocity in feet per second, equals 4·1, and whose semi-minor axis is equal to 1·72 on the same scale. Now, in order that we may read the coefficients of various moulinets, from this one curve, the same must be read off, each according to its proper scale, or arithmetically, the ordinates of the curve must be first multiplied for each moulinet with a constant, empirical and calculated number, and besides this, in certain cases, the axis of Y must be moved along the axis of X, by a certain constant, so as to bring the two curves to cover each

other. Mr. Henry has completed this investigation for four different moulinets, with the following results.

In Table II., are given the values for the two above described moulinets, and for a third one described in Morin's Hydraulique, p. 100, et sequ., and also mentioned in Weisbach. In the table it is called the Lapointe moulinet, after its inventor. Table III. gives the values for a Woltman moulinet described in the Annales des Ponts et Chausseès, November and December, 1847, by Baumgarten. The number of revolutions which form the bases of the coefficients, were found for the cup, propeller, and Woltman moulinets by direct experiment; for the Lapointe moulinet they were calculated, but likewise from experimental data which may be found as above indicated.

TABLE I.

Velocity in ft. per sec.	Cup-Moulinet.		Propeller-Moulinet.	
	Revolutions per sec.	Coefficient.	Revolutions per sec.	Coefficient.
0·3	0·0000	....	......	......
0·5	0·0391	12·778	......	......
1·0	0·0900	11·123	0·558	1·757
1·5	0·1461	10·268	0·872	1·680
2 0	0·2057	9·722	1·213	1·629
2·5	0·2715	9·208	1·514	1·584
3·0	0·3375	8·888	1·897	1·562
3·5	0·4050	8·638	2·229	1·550
4 0	0·4657	8·589	2·635	1·544
4·5	0 5292	8·504	2·947	1·542

A few still greater velocities are given for the Lapointe moulinet, and the corresponding coefficients plot on both sides of a tangent to the ellipse, that is, a perpendicular to the minor axis.

This table is not so satisfactory as Table II., which is perhaps accounted for by its being based on a smaller number and less complete series of observations than Table II.

In looking at the subject from a purely practical point of view, the question naturally arises, what value these and previous investigations with respect to the coefficient of a moulinet, really have.

TABLE II.

Velocity in ft. per second.	Ordinates of Ellipse.	Coeff. of Cup Moulinet.			Coeff. of Prop. Moulinet.			Coeff. of Lapointe Moulinet.		
		Observed.	Computed.	Diff.	Observed.	Computed.	Diff.	Observed.	Computed.	Diff.
0·3	0·000	......	14·573	....	......	......	....	......	......	....
0 5	0·431	12·778	12·704	+ 0·074	......	......	....	......	......	....
0·65	0·694	......	......	....	......	1·880	....	......	......	....
1·	0·961	11·123	11·190	— 0·067	1·757	1·744	+ 0·013	......	......	....
1·5	1·214	10·268	10·300	— 0·032	1·680	1·673	+ 0·007	......	0·650	....
2·	1·395	9·722	9·662	+ 0·060	1·639	1·627	+ 0·002	0·571	0·572	— 0·001
2·5	1·524	9·208	9·208	0·000	1·584	1·595	— 0·011	0·546	0·540	+ 0 006
3·	1·617	8·888	8·881	+ 0·007	1·562	1·572	— 0·010	0·519	0·519	0·000
3·5	1·678	8·638	8·686	— 0·048	1·550	1·556	— 0·006	0·507	0·502	+ 0·005
4·	1·712	8·589	8·546	+ 0·043	1·544	1·545	— 0·001	0·496	0·493	+ 0·003
4·5	1·720	8·504	8·518	— 0·014	1·542	1·540	+ 0·002	0·486	0·485	+ 0·001
5·	......	......	......	....	......	......	....	0·477	0·480	— 0·003
5·5	......	......	......	....	......	......	....	0·474	0·478	— 0·004
		Sum.......		+ 0·015			— 0·004			+ 0·007
		Mean....		+ 0·0017			— 0·0005			+ 0·0009

Regarding the comportment of this coefficient, there is a difference of opinion. Hagen, one of the most eminent of German Hydraulic Engineers (see Wasserbau, II., 1 p. 257), and Bauernfeind (see Vermessungskunde p. 377, et sequ.) and Morin, hold that the number of revolutions per second of two of the above moulinets, are directly proportional to the velocity of the current, that is to say, each moulinet has a certain peculiar coefficient, which may be used in measuring all velocities, and consequently ought really to be found and indelibly engraved on the instrument by its maker,

Of an opposite opinion, speaking of the same two moulinets and two others, are Baumgarten (Annales des P. & C. Nov. et Dec. 1847), Weisbach (Vol. II.) and Mr. Henry, who claim to have found that this coefficient varies with the velocity measured.

TABLE III.

Velocity in ft. per sec.	Ordinate of Ellipse.	Coefficient of Woltman Moulinet (according to Baumgarten).			
		Observed.	Selected and mean.	Computed.	Difference.
1·05	......	......	......	1·525	......
1·063	......	1·606	......	......	......
1·138	0·352	1·493	1·493	1·477	— 0·016
1·325 } 1·402 }	0·656	1·405 } 1·445 }	1·425	1·435	+ 0·010
1·617	0·798	1·368	1·368	1·416	+ 0·048
2·139 } 2·188 }	1 177	1·364 } 1·324 }	1·344	1·364	+ 0·020
2·575	1·366	1·306	1·306	1·343	+ 0·037
2·835	1·421	1·342	1·342	1·331	— 0·011
3 845	1·631	1.291	1·291	1·302	+ 0·011
6·263	1·720	1·305	1·305	1·290	— 0·015
Sum					+ 0·084
Mean					+ 0·0105

If we now, as has usually been done, express the relation of the number of revolutions to the current velocity by several coefficients and a formula, then the mere thought that it is necessary to enter into a long calculation to find the velocity corresponding to each reading of the instrument, induces one to abstain from the use of such an apparatus, and this is perhaps to a great extent the very reason why the moulinet has been so little used in comparison to floats and other less accurate objects for measuring velocities.

What is wanted, is the means of knowing at once what each reading of the moulinet signifies when expressed in feet per second, and

the setting up of a curve, or what is the same thing, of a law for the relation between the number of *revolutions* and the velocity or between the *coefficients* and the velocity, is an outside matter and a *departure* from the object sought.

Again, should any one be called on, say, to testify in court respecting any measurements in which a moulinet had been used, he could not, after all, rely on computed values; but should he have made direct experiments, at different velocities, like those of Mr. Henry with the identical instrument, he could then say with certainty just what velocities had been observed.

If we plot the number of revolutions of the moulinet in one minute* as ordinates, and the corresponding velocities expressed in feet per second as abscissæ on good diagram paper (paper divided off into squares), and choose a proper scale, then the velocities corresponding to every five whole number of revolutions per minute, may be read off and tabulated at once, without committing an error $> 0{\cdot}05$ feet per second, the more so since the curve thereby formed is a very regular one, and coincides almost exactly with the given points. Now, interline the differences and equalize them, and we will get finally a table of velocities in feet (to three places of decimals) per second corresponding to every whole number of revolutions of the moulinet in one minute and ranging, to take the example of a table for a Woltman moulinet, I had occasion to make last Fall, from 10—320. A Woltman or any other moulinet is incomplete without such a table, and is then regarded as an object of curiosity, more frequently, than as an instrument of service.

It is now an easy matter always to let the moulinet register when taking the observations, a minute, an aliquot part, or an even multiple of a minute, and then the corresponding velocity may be at once found from the table. If the moulinet is allowed to register twice in succession for a half minute in one place, then the sum of these two readings gives the mean of two minute readings, and the table gives us the observed velocity. We need now only divide the cross section of the river or canal into parts that contain a whole and *convenient* number of square feet, measure the velocity in the centre of each of these, and then a series of *mental* multiplications and *one* addition suffices to give the total quantity in cubic feet per second.

As above mentioned, the smaller the partial areas, the more accurate will be the measurement. A variation in the observed number

* Why a *minute* is chosen, will appear presently.

of revolutions is caused quite as likely by the want of uniformity in the flow of water at any one point, as by an error of observation, provided the measurement is conducted with due and proper care. In canals, such variation between two consecutive readings should never exceed five per cent., equal to a variation in the velocity of about four and a half per cent., but as a rule it will be found to be either much less or nothing at all.

To return to a consideration of the instrument itself—of moulinets in general—it is to be regretted that some good maker of instruments has not ere this manufactured them, and supplied each instrument with its proper, as above described, table. Inasmuch as a practising engineer is not usually called upon to do anything more than *use* an instrument as it is furnished him by the maker, it ought still less to be expected of him that he should institute a long and extensive series of experiments with every purchased moulinet, before the same can be of any service to him. Besides, he has frequently no time to do so; he is called upon, say late in the Fall, to gauge the flow of water in a canal, at a time when he wakes up every morning in the expectation of finding a three-inch coating of ice over the scene of his operations. Such experiments are also expensive and troublesome. An instrument maker could choose his own time, and could procure a boat, arranged so as to be dragged through a pond or lake, with instrument properly attached, with various velocities, once for all.* And hence it would seem from all this, that these instruments, whether arranged to register telegraphically, or so arranged as to slide up and down along the rod that holds them,† and each one furnished with a proper table of velocities, will, in the future, be used much more frequently than they have been used hitherto.

Boston, March 23, 1869.

* If all moulinets were made precisely in accordance with a fixed pattern or model, it would suffice to make these experiments, with only two or three such, carefully and accurately; the results thus obtained could then be used for all the others.

† When, as has usually been taught, the rod together with the instrument attached is to be lifted up out of the water at every reading, this operation causes even in measuring in a depth of only ten feet, a great deal of labor, loss of time, and danger of injury to the moulinet itself. The whole apparatus could, however, be easily so arranged, that the heavy rod is always to remain upright on the bottom, while only the moulinet is raised out of and lowered into the water, along this rod, at each reading. For small measurements, in mill flumes, etc., an apparatus of this kind is perhaps even preferable, being more portable, to a Henry telegraphic register.

EXTRACTS FROM AN ENGINEER'S NOTE BOOK.

By W. M. Henderson, Hydraulic Engineer.

(Continued from page 247).

On the Strength of Steam Boilers.

The tensile strength of good iron boiler plate, at 80°, is about 56,000 pounds per square inch. Its tenacity will be increased as the temperature rises, under the conditions of generating steam, up to 550° above the freezing point; its maximum strength then is 65,000 pounds per square inch. From this it decreases in direct proportion; at double that temperature it loses one-half. As the temperature of the water in a steam-boiler rarely exceeds 400°, its application to their construction is especially favorable. For the purposes of calculation its average strength may be assumed to be equal to 60,000 pounds per square inch. The deductions to be made for single riveted plates is 44 per cent, for double riveted plates 30 per cent., or, ratio, plate being 100: single riveted 56, double riveted 70. From this data, the bursting pressure, equivalent to the ultimate strength of the single riveted joint, is reduced to 34,000 pounds per square inch, and the double riveted joint, similarly, to 42,000 pounds. It is commonly believed the plates are strongest in the direction of the fibre, but experiments have proved they are about 2½ per cent. stronger crosswise of the fibre; it may therefore be safely assumed the strength of boiler plates are equal in all directions. The strength of the plates will be increased by the amount of hammering and drawing through the rolls they receive, and a decided increase of strength will be obtained by cold rolling under pressure. On the other hand, cold hammering is highly injurious, causing crystallization, and, consequently, impoverishing of the material. After such treatment the plates should be re-heated, and allowed to cool slowly, to recover their strength. The strength of cylindrical boilers subjected to internal pressure vary inversely, as their diameters; whilst there seems to be no general rule regarding variations in their length, the strength is affected so slightly that it may be almost entirely disregarded in practice. The strength of cylindrical flues subjected to external pressure vary inversely as their diameters, and also as their length. In the case of the pressure acting internally, the material of the boiler shell is extended equally throughout all its parts, and its cylindrical form is main-

tained at all stages of the pressure. This is very different when the pressure acts externally; the material being compressed, loses its cylindrical form by crumpling up in longitudinal lines near the middle of its length. This comparatively small portion of the tube then has to resist the main force of compression, since it will be seen the ends of the tube are rigidly held in position by the inflexible heads of the boiler. The pressure producing this collapsing force is always proportional to the longitudinal section of the flue, whilst the part where the collapse will take place is, to a certain extent, independent of the same. To meet this disparity of strength, the flues should be divided into shorter length, increasing their strength in uniformity with that of the exterior shell of the boiler, by riveting at intervals to the joints, T, or angle iron rings, or by constructing them entirely of corrugated iron. This equalization of the powers of resistance of the different parts of a steam boiler is of the utmost practical importance, as any increased strength of the outer shell is of no absolute value, so long as the internal flues are liable to be destroyed by collapse at a pressure less than half that required to burst the shell which envelopes them, the extra thickness of metal in the shell being so much material thrown away, adding nothing whatever to the strength.

In the construction of steam boilers, it seems to have been tacitly admitted that the flues, if not the strongest parts, were at least quite equal in strength to the outside shell, and this would appear to be the case, judging by the natural course of analogous reasoning, such flues being of greatly reduced diameters, in proportion to the shell, and generally constructed of the same thickness of plate. It was only from the occurrence of frequent explosion by their collapse, that attention was directed to the existing discrepancy, which led to a series of experiments, conducted by Mr. Fairbairn, the result of which established the fact that the flues in the Cornish and double-flue description of boilers, were by far the weakest part of the construction, in many cases being only one-third as strong as the outside shell. Another matter developed in the course of these experiments, and one that even now is generally disregarded, is the manner of constructing the longitudinal joints of flues, by lapping them over each other; such a departure from the true circle, which can easily be maintained by making a *butt* joint, with longitudinal covering plates, will impair to a considerable extent their powers of resistance—the loss of strength sustained from this simple matter

has been ascertained to be as 7 is to 10, or nearly one-third. In every construction where tubes have to sustain an external pressure, the cylindrical is the only form to be relied upon, and any departure from it is attended with danger. Elliptical tubes should never be used, as their powers of resistance are reduced in proportion to the amount of departure from the true curve.

In the designing and constructing of steam boilers, care should be taken to so proportion the various parts that they may be, as nearly as practicable, equally strong in every direction. The flues, as has been shown, will require especial attention, being the weakest parts, as at present constructed. Flat surfaces are known to require staying, and the necessary strength can be readily secured by calculation. Still, after all this, a cylinder boiler will be, estimately, just twice as strong, in the longitudinal direction, as in the curvilinear, from a mathematical principle involved in the construction of all cylindrical vessels, *i. e.*, "the areas of circles are to each other as the squares of their diameters." The material offering resistance to rupture in the longitudinal direction being the area of the rim section of the shell, as against the pressure exerted on the area of the boiler head. In the curvilinear direction, it is the area of the longitudinal section of the plates, passing through the axis of the boiler, as against the pressure exerted on the area of the enclosed section. From this, it will be seen that boilers having increased dimensions should also have increased strength in the ratio of their diameters, *i. e.*, one boiler twice the diameter of another should have double the thickness of plates, having to resist double the pressure in the curvilinear direction, and the heads being increased in the proportion of four to one, quadruple the pressure in the longitudinal direction. That this ratio of progression is correct, will be evident by considering that the circumference is doubled with the diameter, and the thickness of the plate also being doubled, gives four times the area of the previous cross section, the power of resistance increasing in strict uniformity with the demand. I have previously stated that the strength of steam boilers, in their longitudinal direction, is, *estimately*, twice that of the curvilinear: it would be precisely so, were it not for the manner in which many cylindrical boilers are constructed. The shell being composed of short cylinders, formed from one plate, with one row of rivets joining the single lap, here it will be seen one side of the plate remains intact, while where these short cylinders are riveted, one to

the other, the full percentage of rivet holes are punched, or drilled out, giving an increase of strength in the curvilinear direction equal to about the double riveted joint, reducing the excess of strength possessed in the longitudinal direction, from 2 to $1\frac{1}{2}$, and by double riveting the longitudinal seam, this will be further reduced to about $1\frac{1}{3}$.

In regard to the strength of the flat stayed surfaces of steam boilers, it has been found, by actual experiments, that at a temperature of 388° Fahr., or 80 pounds steam pressure, it required a force of 8·1 tons to draw out a $\frac{3}{4}$ iron stay screwed into a $\frac{3}{8}$ copper plate, stripping the thread in the latter; 10·7 tons to draw out a $\frac{3}{4}$ iron stay screwed and riveted into a $\frac{3}{8}$ copper plate; and 12·5 tons to draw out a $\frac{3}{4}$ iron stay screwed and riveted into a $\frac{3}{8}$ iron plate.

Ratio Iron and Iron screwed and riveted	1000
" and Copper " " "	856
" " " "	648
Copper and Copper screwed and riveted	576

Philadelphia, April 5th, 1869.

(To be continued.)

ENGINEERING ARCHITECTURE.

By Alf. P. Boller, C. E.

Custom has so entirely separated the professions of the engineer and architect, that the relations between the two are practically lost sight of. The dividing line it is difficult to lay out rigidly, so insensibly does one merge into the other. It is not desirable that either profession should be an accompaniment of the other in the same individual, but it is important that their relative bearings should mutually be better understood. There are many principles in common, which, felt and realized by the engineer and architect, would elevate their respective professions to a higher degree of usefulness. There is no reason in the world why our engineering works, as a rule, should be as devoid as they are of any effort to attract as works of art; or, on the other hand, that modern architecture should so uniformly violate its high position by an unmeaning decoration of often faulty construction.

It is, indeed, not such a long time back that both professions were practised by the same individual, and occasionally painting

and sculpture were added accomplishments. Since then, the progress of a material development, necessitated by an advancing civilization, opened up such a diversified field of intellectual culture as to compel the gradual separation of the sciences and arts. More especially is this true of the latter part of the last and beginning of the present century, when canal, harbor and railway improvements, changed the whole system of commercial intercourse, and made a new people of most of the nations of the earth.

Men made specialties of the several classes of internal improvements, and now engineering is subdivided into distinct branches, as civil, mechanical, hydraulic engineering, and the like, while architecture has to do exclusively with the erection of buildings in conformity with modern requirements, and calls in general terms for simply good construction, combined with good taste. It would be an interesting, and perhaps a profitable speculation, to inquire in what manner the separation of the professions may have produced what is popularly called the "degeneracy of modern architecture;" but it will be more appropriate in this place to consider the result of this separation upon our engineering profession, and to press the importance of giving more attention to such architectural principles as enter into engineering constructions. In an article like this, we can but deal in generalities, which may serve to call attention to an important and too much neglected topic. The treatment of any abstract subject, like the "Æsthetics of Construction," brings us at once into difficulties at its very threshold. The trouble lies in the fact that we cannot all occupy, without much study, the same standpoint of criticism in a science consisting wholly in sensations, involving mental and moral qualities and conditions, that are different in different men. In other words, it is a heart matter, requiring education and practice to give it outward forms and shapes. Be this as it may, there is no question but that engineering education, in this country at least, almost entirely ignores its architectural bearing, and although there is no real excuse for this neglect, one can readily perceive the cause from which it springs. Whether architecture is looked upon as a ramification of engineering, or engineering a branch of architecture (in which case we must give to architecture its widest meaning), modern practice has delegated the engineer to be a pioneer of society and civilization, bending and utilizing the forces of nature to the service of mankind, while the architect follows in his wake, and endeavors to

cater to the refinements of man, in speaking to his soul through an infinity of beautiful forms and colors, and making it rejoice with the elevating influences of pure art. The result of such division is perhaps natural, in that the one accomplishes his ends without any thought of how his work may be a source of pleasure to his fellows, while the other neglects his proper construction for his "art," and fails in his "art" for the lack of good construction. He forgets that one must supplement the other. The one violates good taste, while the other defies good sense. The engineer has a slight advantage over the architect, for he can have good construction and no art, while the architect cannot get good art without good construction to base it upon. The neglect of this harmony shows in engineering works by a certain harshness and rigidity of form, and an ignorance of the power or relation of color, while architectural works lean toward an effeminate and unskillful construction, covered up by an unmeaning or meretricious ornamentation. There is no reason why our public works, our viaducts, bridges, station buildings, and the like, should not be works of art, and not only specimens of constructive skill. It is the throwing of the heart into his work, as well as the head, that we ask of the engineer, and to appeal to men's sense of the good and beautiful, of truth and honesty, as well as to their feelings of wonder at the constructive power that may be evinced.

Mr. Ruskin, in his "Stones of Venice," bases his architectural criticism and appeals upon the axiom, "that there can be no architecture without building, and no *good* architecture without *good* building;" and he uses the word "building" in the sense of "construction." Now, pass the architectural monstrosities of the present day under review. Apply this truism, and then see why we have such bad architecture. The ornamentation or decoration does not grow out of the necessities of the case, but is a great staring burlesque on the construction it tries to conceal. It is in the essential fitness of things that ornamentation becomes pleasing and appropriate, and it is not in plastering a building or any public work with Grecian columns or pilasters, or Gothicising all the arches, that makes "architecture;" and yet a large proportion of the practitioners in this country would have us think so. Again, ornament ought never to be constructed, that is, artistic forms and graceful shapes should never be put on construction, simply because they are such, for the moment that is done, such forms and shapes

cease to become artistic and graceful. The character of the ornamentation should be expressive of a purpose existing in the work. It has the same relation to building (and by building we mean *all* building, whether a bridge, viaduct, or a house) that poetry has to prose. The latter narrates the facts, in language however rough, the former clothes it with beauty and grace, and expresses passion or sentiment, grandeur or sublimity. Why should not the engineer bring these principles of architecture to bear in his own work? Surely, they eminently belong to him, and especially so since the various ramifications of the professions, in the fullest sense, has compelled him to select specialties. The constructive engineer rarely, nowadays, performs the duties of the surveyor, but devotes his time and energies to the execution of bridges and viaducts, the construction of piers, lighthouses, warehouses, and at times the building of ships. It is to these divisions of the profession that our subject ought to be of much interest, and its proper study would result in public works that would be monuments of beauty or grandeur, as well as specimens of constructive skill. In almost all engineering architecture, simplicity of ornamentation is demanded, and, in truth, there are not a few public works in this country that would take but little to have made them truly architectural. There is no need to waste time or labor in a flippant carving or sculpture, especially so since engineering works susceptible of architectural effect are usually seen at a distance, and the beholder is mostly affected by a pleasing unison of parts and of color. In many works, the simple scientific construction and *proportion* of parts, necessary for the performance of the duty of the structure, is all that is required for a pleasing effect, for proportion is a vital element of what we call beauty. Then, again, engineering works are often characterized by a certain massiveness, engendering a feeling of strength and security, which ought never to be dissipated by light, inappropriate mouldings or carving. To extend the practical bearing of the remarks made in this article, will require the writer to trespass upon the space of the *Journal* in a future number.

PUMPING ENGINES.

By Henry P. M. Birkinbine, C. E.

The *Journal* has done good service by giving so much of its space to the discussion of the relative value of different forms of pumping engines.

The articles already published, as well as the practice of engineers, show that upon this subject there is a great diversity of opinion. An engineer of prominence in this country, in a report upon the water supply of a city, makes the following remarks upon pumping engines: "Much examination has been given to this subject; yet much uncertainty remains. Fortunately this uncertainty, so far as the interests of large cities are actually concerned, is of no great practical importance, notwithstanding so much has been said and written to prove the contrary." The earliest utilization of the power of steam was to raise water, and great numbers of engines are now in use for this purpose, in draining land and mines, supplying cities, etc., and are required for many engineering operations. There should, therefore, now be no ignorance or uncertainty upon the subject; surely, there are some forms better adapted than others.

In the closing paragraph of an article on "Cornish Pumping Engines" in the *Journal*, Vol. 47, p. 16, the writer says: "I will only add that there is yet another engine based upon reciprocating parts, without a fly-wheel, which has the capacity of rivalling the Cornish engine in its best results, without the requirements of magnitude—almost immensity—which the Cornish engine demands." If the writer of this article really knows of such an engine, he certainly owes to the profession and the community to give a full description of it, for in the article alluded to, he credits the Cornish engine with a duty of 130,000,000 foot pounds. (He no doubt means 1,300,000 foot pounds.) There is no record of any other form of engine producing so great a duty under the most favorable circumstances.

From the brief description given, we are left in ignorance as to the engine referred to, and as he has not enlightened us, we may be excused for taking the Yankee privilege of *guessing* that he refers to that class of direct acting engines (of which that known as Worthington's patent is the most notorious, though not the best). This *guess* is made because it answers the description, and because

the maker actually claims a high duty for his engine, and professes to have produced results which are truly wonderful when the peculiar construction of the engines is taken into consideration. Having never heard any of the other makers of this class of engine claim extraordinarily high duty for them, it may be well to examine somewhat into these claims.

The authorities of this city have been induced to contract for two of these engines, upon the statement that they produce a higher average duty than Cornish engines. (See Appendix to Journal of Select Council, 1868, page 237.) In the experiments made with the engines of the former class at Cambridge Water Works (see reports upon pumping engines, 1857 and 1859, page 76), a duty of 712,784 foot pounds is claimed. For a pair of engines of this class, at the Charlestown, Mass. Water Works, a duty of 774,445 foot pounds is claimed.

While this is far less than your correspondent gives to the Cornish Engine, still these results, if true, are most extraordinary, considering the many disadvantageous peculiarities for high duty of this class of engine.

So much for the duty claimed. In the Cambridge City Documents, page 137, there is a statement of the expense of running these engines for one month (made as a comparative test to show their economy), when 52,800 gallons of water were raised one foot high by one pound of coal; this gives a duty of but 440,000 foot pounds. This is more than could be expected from such a form of engine, although it is much less than the duty claimed for it. The peculiarities by which it is *claimed* that the high duties of this form of engine are produced, are, the duplicating of the pump and engine, and arranging the duplex machinery so that one engine works the valves of the others, thereby producing a comparatively continuous flow from the pumps, and high degree of expansion, particularly in those having double steam cylinders.

The only possible advantage of duplicating the engines is a more steady flow of water in the delivery pipe; but as duplicating the parts increases the friction, and there is unavoidable loss of steam in ports, cylinder ends, etc., it certainly can effect no actual gain in duty.

The stroke of engines of this class is of necessity very short; a double acting pump of the diameter that experienced engineers would make stroke of five (5) feet, is rarely over two (2) feet, thus

necessitating the more frequent stopping and starting of the water at the commencement of each stroke, another element impairing their efficiency.

Much stress is laid upon the economy produced by the expansion, particularly when the double cylinder or Wolf engine is used; but by this there can be but little gained, as there is neither fly-wheel, beam, or other heavy moving parts in which to store the excess of power in the earlier stages of the stroke, and utilize it towards the close of the same; the only reservoir of force being the water in motion and the difference between the power necessary to start the column of water, and to keep it moving. The terminal pressure upon the piston in these engines must be sufficient to keep the column of water in motion, whereby a large amount of force is lost which may be utilized in engines with fly-wheels, or other heavy moving parts.

In practice, very few of this class of engines make their full journey; frequently there is a large amount of steam which cannot be used, left in each end of the cylinder. From the above, it is evident that such an engine which gives an actual duty of four hundred thousand (400,000) foot pounds, is truly a remarkable machine.

So far as the writer has been able to examine them, none have produced an actual duty of two hundred thousand (200,000) foot pounds which (considering the peculiarities of their construction), is a fair average duty. It is doubtful if any of them, no matter by whom constructed, can exceed that duty.

The engines of this class are found to be valuable where a small amount of water is to be raised, or where they are only used occasionally.

One form is extensively employed in the Pennsylvania anthracite coal mines, and is found to be a desirable arrangement; the pump being placed at the bottom of the mine, long connecting rods are not required, thus saving the friction incident upon carrying power so great a distance. The coal used having but a nominal value, economy of fuel is not an object. But where an amount of over one million of gallons of water per day is to be raised one hundred feet, the question of fuel becomes the one of the greatest importance.

From a careful examination of the recorded duty, and a personal inspection of many of the various forms of pumping engines, the

following facts upon the subject of duty in foot pounds have been collected.

	Highest Duty claimed.	Average Actual duty.
Cornish Pumping Engines	1,300,000	550,000
Condensing Pumping Engines with fly-wheel	700,000	350,000
High Pressure Pumping Engines with fly-wheel	250,000	200,000
Direct Acting Engines of the Worthington type	750,000	200,000

By making an estimate (from the above actual duties) of the quantities of coal required to raise, say, five million gallons one hundred and fifty feet high, the relative value of the different forms of pumping engines will be apparent. It is as follows:—

Cornish Engine	11,427	pounds of	coal.
Condensing fly-wheel engine	17,959	"	"
High pressure fly-wheel engine	31,425	"	"
Direct acting engine, as above	31,425	"	"

The other items of running expenses are about in the same ratio; the first cost being the only element in which a large difference could exist. The value of engines is much controlled by the style of finish and peculiarities of construction. Thus, the cost of an overhead beam Cornish pumping engine is very much greater than that of a Bull or upright Cornish engine of the same capacity and efficiency.

The saving of fuel alone will indicate that at a liberal estimate for the cost of the first two styles of engines mentioned, the last two would be dear at any price.

Patents and royalties have no doubt much to do with the apparent diversity of opinion as to the best form of pumping engine; some of them would most certainly never be considered, if not pressed into notice by those having pecuniary interests in their success. The authorities of one of our important cities, requiring a large supply of water, were actually induced to take into serious consideration the adoption of a patented form of rotary steam engine and rotary pump, and had not a prominent citizen conversant with such matters, exerted himself and enlightened the public, the construction of a four hundred and fifty (450) horse power rotary

pumping engine would have been attempted, for which the patentees promised excessive duties, and many other advantages over all other forms of pumping engines. The pertinacity with which many patents are urged upon the public, the combinations of men and money formed to press them, and the difficulty of arriving at the actual facts in regard to the duty and efficiency of the various forms of pumping engines, make it important that the subject be thoroughly understood, so that the best and most efficient form of engine may be adopted.

BELTING FACTS AND FIGURES.

By J. H. Cooper.

(Continued from page 238.)

"Among the more recent improvements in the way of *transmitting power* for long distances, is the substitution of belts by endless wire-ropes, running at a high speed; their use bids fair to add immensely to our manufacturing facilities. The distance to which you can thus transfer power ranges from 75 feet to 4 miles. Just where the belt becomes too long for economy, there the rope steps in. In place of a flat faced pulley, a narrow sheave with a deep flaring groove is used, the groove being filled out, or lined rather, with leather, oakum, india rubber, or some other soft substance, to save the rope. The essential points are a large sheave, running at a considerable velocity, and a light rope.

When the distance exceeds 400 feet, a double grooved wheel is used, and a second endless rope transmits the power 400 feet further, and so on indefinitely. The loss by friction is about 8 per cent. per mile. A few examples may prove of interest, and give information.

It is required to transmit 300 horse-power by means of a wire rope. A wheel $14\frac{1}{2}$ feet diameter, making 108 revolutions per minute, is sufficient; the rope running at a rate of 4920 feet per minute—size of rope required, one inch diameter. The distance has nothing to do with it. Again: "It is desired to transmit for any distance as much power as a 12 inch belt will give." Assuming that the belt travels in the neighborhood of 1300 feet per minute, it is about equivalent to 20 horse power, and a grooved sheave of 7 feet diameter running 100 revolutions per minute, with a $\frac{5}{8}$-inch

rope, will be the proportions required. Again, a 4 feet wheel, running 100 revolutions per minute, with a $\frac{3}{8}$-inch rope, will convey from 4 to 5 horse-power. The cost of the rope is always the smallest item, amounting to a few cents per foot, and not one-tenth the cost of an equivalent amount of belting.

"One is thus enabled, at a small expense, to transmit power in any direction; for instance, to a building lying remote from the main factory buildings, where it is not worth while to put up a separate engine. Across rivers, creeks, canals, streets, over the tops of houses, under water, from cellar to roof, etc.

Frequently an excellent site for water power remains unimproved for want of suitable building sites in the neighborhood. The water may be conveyed *down stream* by means of expensive canals and flumes; but by a wire rope transmission we can transfer it in any direction, either up stream, across it, or sidewise, up and down grades of one in eight—in fact, anywhere.

"In many sections of our country coal is dear and water power plenty, but not improved, for reasons which may be set aside by the above method. In Europe, over a thousand factories are driven in that way."—*The Manufacturer and Builder*, Feb. '69, p. 38.

In a paper by Mr. John Ramsbottom, in *Newton's Journal*, Vol. XXI, p. 46, on traversing Cranes at Crewe Locomotive Works, dated January 28, 1864, mention is made of the means by which power is communicated from the shop lines of shafting to the gear of the cranes.

"It consists of a $\frac{5}{8}$-inch diameter soft, white cotton cord, weighing about $1\frac{1}{2}$ ounces to the foot, running at the rate of 5000 feet per minute, in a line with the longitudinal motion of the crane, above the same and over a 4 feet diameter tightener sheave. This sheave is weighed so as to put a tension on each strand of the cord of 108 pounds, which is found to be the best working strain for keeping the rope steady, and giving the required 'hold' on the main driving pulley.

The cranes have a span of 40 feet 7 inches, a longitudinal traverse of 270 feet, and the rails are 16 feet above the floor.

The cord is supported every 12 or 14 feet by cast iron fixed slippers of plain tough section, $1\frac{3}{8}$-inch wide, with side flanges. These slippers are placed $1\frac{1}{2}$ inch below the working line of the driving side of the cord, so as to allow the driving wheels on the traverser

to pass them; they are not oiled, and the friction of the cord in them amounts to two-fifths of the working load.

Motion is communicated to the gear of the crane by pressing the cord into grooved cast iron pulleys. The grooves in the driving pulleys are V shaped, at an angle of 30°, and the cord does not touch bottom; the guide pulleys have circular grooves, same diameter as the cord, and the pressure pulleys have a circular groove of larger diameter than the cord. The driving pulleys have a diameter equal to thirty times the diameter of the rope. Guards are put on the pulleys to keep the ropes in.

The driving power of the cord to lift 25 tons is only 18 pounds, irrespective of friction, which is a ratio of 3111 : 1. Light loads are about 800 : 1. In the gib cranes, driven by similar means, the ratio is 1000 : 1 when lifting 4 tons at the rate of 5 feet 1½ inch per minute.

The actual power required to lift 9 tons, besides the snatch block and chain, has been found to be 17 pounds at the circumference of the driving pulley. The crab, when unloaded, requires 1⅛ pounds to overcome its friction.

The cords are soon reduced to $\frac{9}{16}$ inch diameter, and last about eight months at constant work.

In an overhead traverser, used in the boiler shop, lifting 6 tons, three years in use, a ⅜-inch cord was employed, but was afterwards changed for a cord ½-inch in diameter.

The light driving cord is the only plan compatible with high speeds; a heavy chain, belt or cord would soon wear out and break by its own weight.

Centrifugal Pumps.—The great Appold centrifugal pumps, to be worked in connection with Mr. Hawkshaw's important work, the Amsterdam Ship Canal, are to lift 2,000 cubic metres, or, say, 440,000 gallons of water per minute. The lift is not great, but for each foot of lift, the actual duty, irrespective of all losses of effect, is 133⅓ horse-power.—*Engineering.*

Mechanics, Physics, and Chemistry.

ON THE SOURCE OF LIGHT IN LUMINOUS FLAMES.

By Edward Frankland, F.R R.

(Professor of Chemistry, R. I.)

The most prolific source of error amongst mankind is the unquestioning acceptation of authoritative opinion. However much we may pride ourselves upon the sifting of the explanations of things by our own enlightened judgments, it cannot be denied that the *ipse dixit* mode of settlement is still wonderfully frequent amongst us. Not only is this the case with the public in general, but even the cultivators of science are not entirely innocent of the same weakness.

The essential difference between a fact and a theory is not always appreciated with sufficient vividness. The statement that "16 parts by weight of oxygen unite with 2 parts of hydrogen to form water," is considered by many, for instance, as perfectly synonymous with the assertion that "1 atom of oxygen unites with 2 atoms of hydrogen to form water."

The existence of an imponderable ethereal medium filling all space is often regarded as equally certain with the presence of a gaseous envelope surrounding our globe.

The atomic theory and the hypothesis of an ethereal medium are, at present, absolutely necessary, the one to the progress of chemistry, the other to the further development of physics; but neither this circumstance nor the splendid discoveries made by their aid can establish their truth. A mathematician starting from false data is sure to arrive at a false result; but it is far otherwise with theory, for false theories can, and constantly do, conduct to true facts. Thus Columbus's counterpoise theory of the earth led to the discovery of America, although that theory was, nevertheless, essentially false.

The most sober worker in science cannot progress without the assistance of theory to co-ordinate his facts, and to lead him on to further research. It is here that even a false theory is invaluable, and it is only when the theory continues to be held after it has become opposed to facts, that it exercises a prejudicial influence

upon the progress of science. Then it hinders rather than expedites the advance of the experimenter, and ought to be at once abandoned.

In pursuing the investigation forming the subject of this discourse, the speaker had been compelled thus to abandon a theory of the source of light in luminous flames, which he, in common with others, had derived from Davy's classical researches on flame.

Our text-books answer the question, *What is the source of light in a luminous gas or candle flame?* in the most positive and unanimous manner.

Selecting from some of the most celebrated, the following quotations may be made:—

"All our artificial lights depend upon the ignition of solid matter, in the intense heat developed by the chemical changes attendant on combustion."—*W. A. Miller.*

"Whenever hydrocarbons are imperfectly burnt, there is a deposition of carbon, and this temporary deposition of carbon is an *essential* condition for the production of the white light required in an ordinary flame."—*Williamson.*

"The illuminating power of the gas flame is, therefore, due to these *carbon particles*, which are afterwards burned nearer the border of the flame."—*Balfour Stewart.*

"The brightness or illuminating power of flame depends not only on the degree of heat, but likewise on the presence or absence of solid particles which may act as radiant points. A flame containing no such particles emits but a feeble light, even if its temperature is the highest possible."—*Watts.*

The speaker then proceeded to investigate a number of different flames: he showed that there are many flames possessing a high degree of luminosity, which cannot possibly contain solid particles. Thus the flame of metallic arsenic, burning in oxygen, emits a remarkably intense white light; and as metallic arsenic volatilizes at 180° C., and its product of combustion, arsenious anhydride, at 218° C., whilst the temperature of incandescence in solids is at least 500° C., it is obviously impossible here to assume the presence of ignited solid particles in the flame. Again, if carbonic disulphide vapor be made to burn in oxygen, or oxygen in carbonic disulphide vapor, an almost insupportably brilliant light is the result; now, fuliginous matter is never present in any part of this flame, and the boiling point of sulphur (440° C.) is below the

temperature of incandescence, so that the assumption of solid particles in the flame is here also inadmissible. If the last experiment be varied by the substitution of nitric oxide gas for oxygen, the result is still the same; and the dazzling light produced by the combustion of these compounds is also so rich in the refrangible rays, that it has been employed in taking instantaneous photographs, and for exhibiting the phenomena of fluorescence. Lastly, amongst the chemical reactions celebrated for the production of dazzling light, there are few which surpass the active combustion of phosphorus in oxygen. Now, phosphoric anhydride, the product of this combustion, is volatile at a red heat,* and it is, therefore, manifestly impossible that this substance should exist in the solid form at the temperature of the phosphorus flame, which far transcends the melting point of platinum.

For these reasons, and for others which the speaker had stated in a course of lectures on "Coal Gas," delivered in March, 1867, and printed in the *Journal of Gas Lighting*, he considered that incandescent particles of carbon are not the source of light in gas and candle flames, but that the luminosity of these flames is due to radiations from dense but transparent hydrocarbon vapors, As a further generalization from the above-mentioned experiments, he was led to the conclusion that dense gases and vapors become luminous at much lower temperatures than aeriform fluids of comparatively low specific gravity; and that this result is, to a great extent, if not altogether, independent of the nature of the gas or vapor, inasmuch as he found that gases of low density, which are not luminous at a given temperature when burnt under common atmospheric pressure, become so when they are simultaneously compressed. Thus, mixtures of hydrogen and carbonic oxide with oxygen emit but little light when they are burnt or exploded in free air, but exhibit intense luminosity when exploded in closed glass vessels, so as to prevent their expansion at the moment of combustion.

In a communication just made to the Royal Society, the speaker

* Davy mentions this fact in connection with his view of the source of luminosity in flames, and endeavors to explain the (to him) anomalous phenomenon. He says: "Since this paper has been written, I have found that phosphoric acid volatilizes slowly at a strong red heat, but under a moderate pressure it bears a white heat; and in a flame so intense as that of phosphorus, the elastic force must produce the effect of compression."—*Davy's Works*, vol. vi., p. 48.

had described the extension of these experiments to the combustion of jets of hydrogen and carbonic oxide in oxygen under a pressure gradually increasing to twenty atmospheres. These experiments, which were conducted in the laboratory of the Royal Institution, were made in a strong wrought-iron vessel furnished with a thick glass plate of sufficient size to permit of the optical examination of the flame. The appearance of a jet of hydrogen burning in oxygen under the ordinary atmospheric pressure was exhibited. On increasing the pressure to two atmospheres, the previously feeble luminosity was shown to be very markedly augmented, whilst at ten atmospheres' pressure, the light emitted by a jet about one inch long was amply sufficient to enable the observer to read a newspaper at a distance of two feet from the flame, and this without any reflecting surface behind the flame. Examined by the spectroscope, *the spectrum of this flame is bright and perfectly continuous from red to violet.*

With an higher initial luminosity, the flame of carbonic oxide in oxygen becomes much more luminous at a pressure of ten atmospheres, than a flame of hydrogen of the same size and burning under the same pressure. The spectrum of carbonic oxide burning in oxygen under a pressure of fourteen atmospheres is very brilliant and perfectly continuous.

If it be true that dense gases emit more light than rare ones when ignited, the passage of the electric spark through different gases ought to produce an amount of light varying with the density of the gas; and the speaker showed that electric sparks passed as nearly as possible, under similar conditions, through hydrogen, oxygen, chlorine and sulphurous anhydride, emit light, the intensity of which is very slight in the case of hydrogen, considerable in that of oxygen, and very great in the case of chlorine and sulphurous anhydride. On passing a stream of induction sparks through the gas standing over liquefied sulphurous anhydride in a strong tube, at the ordinary temperature, when a pressure of about three atmospheres was exerted by the gas, a very brilliant light was obtained. A stream of induction sparks was passed through air confined in a glass tube connected with a condensing syringe, and the pressure of the air being then augmented to two or three atmospheres, a very marked increase in the luminosity of the sparks was observed, whilst on allowing the condensed air to escape, the same phenomena were observed in the reverse order.

Way's mercurial light was also exhibited as an instance of intense light by the ignition of the heavy vapor of mercury.

The gases and vapors just mentioned have the following relative densities:—

Hydrogen	1
Air	14·5
Oxygen	16
Sulphurous anhydride	32
Chlorine	35·5
Mercury	100
Phosphoric anhydride	71 or 142

The feeble light emitted by phosphorus when burning in chlorine seems, at first sight, to be an exception to the law just indicated, for the density of the product of combustion (phosphorous trichloride) 68·7 would lead us to anticipate the evolution of considerable light. But it must be borne in mind that the luminosity of a flame depends also upon its temperature, and it can be shown that the temperature in this case is probably greatly inferior to that produced by the combustion of phosphorous in oxygen. We have not all the necessary *data* for calculating the temperature of these flames, but, according to Andrews, phosphorus burnt in oxygen gives 5747 heat units, which, divided by the weight of the product from one grain of phosphorus, gives 2500 units. When phosphorus burns in chlorine, it gives only (according to the same authority) 2085 heat units, which, divided as before by the weight of the product, gives 470 units. It is, therefore, evident that the temperature in the latter case must be greatly below that produced in the former, unless the specific heat of phosphoric anhydride be enormously higher than that of phosphorous trichloride. The speaker had, in fact, found that if the temperature of the flame of phosphorus, burning in chlorine, be raised about 500° C. by previously heating both elements to that extent, the flame emitted a brilliant white light.

To return to ordinary luminous flames, the argument of the *necessity* of solid particles to explain their luminosity obviously falls to the ground; and a closer examination into the evidence of the existence of these particles reveals its extreme weakness. Soot from a gas-flame is not elementary carbon, it always contains hydrogen. The perfect transparency of the luminous portion of flame also tends to negative the idea of the presence in it of solid particles. The continuous spectrum of gas and candle-flames does not require,

as is commonly supposed, the assumption of solid particles. The spectra of the flames of carbonic oxide in air, of carbonic disulphide, arsenic, and phosphorus in oxygen, are continuous, and so, as we have seen, is that of hydrogen burning in that of oxygen under a pressure of ten atmospheres. It is to the behavior of hydrocarbons under the influence of heat that we must look for the source of luminosity in a gas-flame. These gradually lose hydrogen, whilst their carbon atoms coalesce to form compounds of greater complexity, and, consequently, of greater vapor density. Thus marsh-gas ($C\,H_4$) becomes acetylene ($C_2\,H_2$), and the density increases from 8 to 13. Again, olefiant gas ($C_2\,H_4$) forms napthaline ($C_{10}\,H_8$), when the vapor density augments from 14 to 64. These are some of the dense hydrocarbons which are known to exist in a gas-flame; but there are, doubtless, others still more dense; pitch, for instance, must consist of the condensed vapors of such heavy hydrocarbons. for it distills over from the retorts in the process of gas-making, Candle-flames are similarly constituted. The direct dependence of the luminosity of gas and candle-flames upon atmospheric pressure, also strongly confirms the view that the light of these flames is due to incandescent dense vapors.

This inquiry cannot be confined to terrestrial objects. Science seeks alike for law in the meanest and grandest objects of creation. From questioning a candle she addresses herself to suns, stars, nebulæ, and comets; the same considerations which have just been applied to gas and candle-flames are equally pertinent to these great cosmical sources of light.—*Proceedings of the Royal Institution.*

A NEW ARRANGEMENT OF THE HOLTZ MACHINE.

By Prof. H. L. Smith.

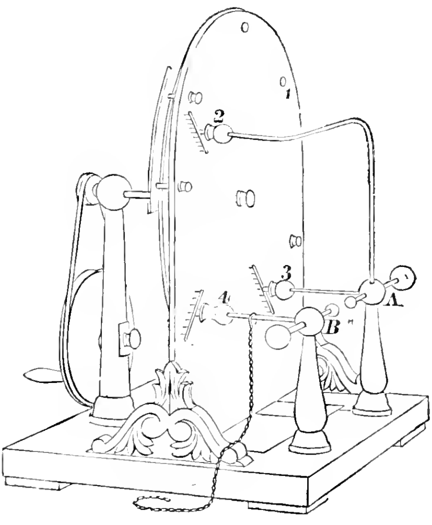
In the ordinary mode of excitation of the Holtz Machine, the knobs of the dischargers are placed in contact, and afterwards separated; the excitation cannot be produced without contact. It follows that, when the balls are separated beyond the striking distance, all action will suddenly cease, and to renew it the balls must again be brought together; and, frequently, in addition to this, a new excitation of the sectors will be required.

A large class of electrical experiments require, virtually, this

separation of the balls beyond the striking distance, *e. g.*, the "electrical flyers," the "head of hair," and the "insulating stool."

The arrangement which I have devised meets this difficulty, and enables us to keep up an unlimited excitation, whatever be the number of points applied to the prime conductor, or whatever may be the distance of the balls of the dischargers; and, in order to excite the machine, it is not necessary that the balls should be in contact.

The arrangement is shown in the following cut. The combs, 2 and 3, are connected with the discharger, A, the one by means of a curved brass bar, the other by a short, straight rod. The prime conductor, which should be large to obtain brilliant effects, and which is not shown in the cut, is placed in contact with the curved bar. Mr. Ritchie places a ball on this bar, with an opening for the insertion of a rod connected to the prime conductor. Opposite to the combs, 2 and 3, are two sectors; that opposed to 2 is shown in the cut. The comb, 4, is connected with the pillar, B, and there is no sector opposed to it. A chain, shown in the cut, connects B with the earth, and it is essential that this connection be as complete as possible, such, for example, as would be made by connecting with the gas pipe. To start the machine, the excited vulcanite may be applied to either of the sectors, 2 or 3; if to the latter, which has the paper tongue of the "feeder" towards the "earth comb," 4, the prime conductor will be charged with positive electricity, and with a 20 or 24 inch plate, sparks $3\frac{1}{2}$ to $4\frac{1}{2}$ inches in length will be seen to pass between the balls of the discharging rods. If the other sector, 2, which has the tongue turned away from the earth comb, is excited, the conductor will be charged with negative electricity, and the sparks will be much shorter and denser. In either case, the length and brilliancy of the spark will depend on the size



of the conductor. When in action, one of the sectors will be +, the other —, and the connection of a + and — sector with the same prime conductor makes the analogy to an electrophorus more clear, the two connected with the conductor representing the upper and under surface of the cover in opposite conditions.

I do not intend, however, to discuss, at present, the theory of this remarkable machine; there is much about it which, with our present knowledge, cannot be satisfactorily explained. The paper tongues may be replaced by needles; and in the arrangement I have described, the sector opposed to comb 3 may be removed (after first exciting the plate by means of sector 2) and the hand held in its place, sparks, not so long, indeed, or frequent, but still sufficiently remarkable, will pass between the balls of the dischargers.

Hobart College, Geneva, N. Y.

THE ELECTRIC SPARK—SOME NEW EXPERIMENTS BY M. J. SEGUIN.

Translated from *Les Mondes*, Vol. XIX., p. 112.

MANY physicists have endeavored to distinguish between the two parts of the induction discharge which are called the halo and spark. The difference between them can easily be increased or diminished by changing the condition of the experiment, as by varying the nature and distance of the electrodes, and the nature and pressure of the surrounding gas. Amongst the most distinct characteristics, there is, besides the difference in appearance, the difference in the action of the two parts of the discharge under the influence of a current of air, or of an electro-magnet, and the difference as seen in a revolving mirror. But all these characteristics are effaced by the variations of the gaseous medium, and it is in this connection that I would here add some experiments to those which I have already described.

1st. Let a spark of one or many centimetres be produced by means of a Ruhmkorff coil between the points of two platinum wires in the interior of a bell jar, which may be exhausted by an air pump. At the first stroke of the piston the halo becomes brighter, and the central spark diminishes in brightness, and at last disappears. The spark is only a luminous jet, of red color, which seems to come from the positive electrode. It is useless to dwell on these changes,

which are well known. But this luminous jet has all the characteristics which belong exclusively to the halo before the rarefaction. I repeat that it deviates by the action of the magnets, and yields equally to the action of a transverse current of air. The current of air is obtained by means of two tubes entering the bell-jar, a large one for exhaustion and a small one through which the exterior air enters. The luminous jet is inflected its entire length as the tube for aspiration is introduced, and it is even divided into two parts by the current of air.

2d. I have observed, with a rotating mirror, the image of the luminous jet when the spark has become effaced. M. Fernet has described the widening which occurs in this case to the blue light of the negative electrode; the red jet which springs from the positive electrode is spread out in the same way, and it is thus seen that the incandesence lasts for some time in all discharges, as M. Lissajous has described in the case of the halo.

3d. The influence of the magnet was tried on a spark produced across the heated column of air which rises above an ordinary flame. The spark has nearly the same appearance as in rarefied air; and if it is made to spring transversely between the poles of an electro-magnet, it is seen to bend on a plane perpendicular to the line of the poles as long as the magnet is in action. The direction of the curve depends upon the direction of the magnetic poles. The spark in this experiment was weak, and in the heated air was only a centimetre long.

4th. As the two parts of the spark pass into each other so easily, it is not astonishing that physicists have not been able to pronounce in a positive manner, when they endeavored to discover whether to attribute to the surrounding medium, or to the substance of the electrodes, the noise of the spark and the light of the halo. Generally they have proved in each of these parts the presence of matter. The preponderating influence depends upon the same circumstances which vary the aspect of the discharge, and yet this has not the same character in all parts of its length. For my part, I have particularly observed the influence of the surrounding gas on the spark, as I have often observed that the noise, the color, and the prismatic characteristic of the spark changes with the nature of the gas. On the other hand, I admitted the intervention of the matter of the electrodes in the halo, after having observed that the spark produced between a platinum wire and an amalgamated cop-

per wire gives in the halo the bright lines due to mercury, and that the halo was also colored with various tints in the sparks which pass between a platinum wire and the surface of saline solutions, to such a degree that a breath expanded the halo into a broad colored sheet, and giving the bright lines due to the base of the salt when viewed with the spectroscope.

These last experiments, repeated under different conditions, very plainly show the influence of the surrounding medium upon the character of the discharge. The platinum wire and the solution are placed as M. Ed. Becquerel suggests, and the solution is connected with the negative pole, as this pole is the most favorable for coloring the spark, which is contrary, however, to a statement I have already given. Then the wire and the liquid are placed in a bell-jar, where the air can be rarefied. By this rarefaction the brilliant spark loses itself in the halo which is colored by the salt, and the color becomes fainter on the upper part of the spark, and draws nearer the solution. The spectral lines of the salt finally disappear, or are only seen intermittently. Other lines than those due to the salt remain in the image of the spark.

M. Ed. Becquerel having just called the attention of physicists to the part which spectrum analysis can undertake in the study of the sparks produced at the surface of solutions of salt, allow me to recall, that in making the discharge, not on the surface of solutions, but as M. Daniel made it, across non-conducting liquids, and adding to these liquids salts in powder, or in solution, I have observed the lines due to the base of each salt.

Inverted Syphon.—An iron pipe, 11 inches in diameter and 8,800 feet (one and two-third miles) long, has been laid in Tuolumne county, California. It runs down a mountain, under a creek, and up the ascent on the opposite side, under a perpendicular pressure at the lowest point of 684 feet.

Water Supply for Jerusalem.—Miss Burdett Coutts has taken on herself the entire expense of introducing pure water into Jerusalem.

THE SALT DEPOSITS AT STASSFURT.

By Messrs. Bald and Mactear.*

The southern portion of the North German Basin is divided by the Hartz into two portions, which are known as the Thuringian and the Magdeburg Halberstader basins, in which salt has been raised for a lengthened period in the form of brine.

The basin covers a surface of 120 English square miles, and is filled with new red sandstone, which is not broken up by any of the older formations. It is interspersed by elevations of gypsum, which is considered a certain indication of the presence of common salt. In the Prussian mine at Stassfurt, in the Magdeburg basin, after passing through 27 feet of alluvial soil, a thickness of 576 of new red sandstone is at once reached, then 213 feet of gypsum, anhydrite and marl, the salt being found at a depth of 816 feet. In the Anhalt mine (half a mile from the Prussian one) the sandstone is entirely wanting, the salt bed being reached at a depth of 480 feet, after passing through 20 feet of soil and 460 feet of gypsum, anhydrite, and marl. The bore-holes at Schonebeck (some miles from Strassfurt) show very distinctly the various strata with which the basin is filled up, as the salts gradually get deeper and deeper. Thus, at bore No. 8, the salt is 1000 feet from the surface, the intervening strata being 200 feet of alluvial soil and 800 feet of new red sandstone. At No. 5 there is 37 feet of alluvial soil, 166 feet of mussel-chalk, and 1277 feet of new red sandstone, the salt being 1480 feet from the surface. At No. 6 there is 30 feet alluvial soil, 877 feet mussel-chalk, and 473 feet new red sandstone, the salt being 1380 feet from the surface. At No. 4 there is 25 feet alluvial soil, 211 feet of what in Germany is called keuper and lettenkohle (literally, copper and letten coal). This keuper is the equivalent of the saliferous and gypseous shales and sandstones of Cheshire, a member of the "Irias" or new red sandstone formation. Lettenkohle is a variety of lignite known in the district as brown coal. Next we have 1067 feet of mussel-chalk, 377 feet of new red sandstone, and the salt at a depth of 1680 feet. Bore No. 3 is somewhat similar to No. 4, there being 30 feet alluvial soil, 435 feet keuper and let-

* Read before the Chemical Section of the Glasgow Philosophical Society, January 18, 1868.

tenkohle, 1087 mussel-chalk, 212 new red sandstone, the salt being 1764 feet from the surface.

In the Magdeburg basin the salt rests on new red sandstone, and in the Thuringian basin on mussel-chalk and magnesian limestone. It is only at Stassfurt and Erfurt that the salt is mined; at all the other places it is obtained by means of brine wells, the liquor from which is concentrated by the graduation process, which consists in allowing the weak liquor to trickle through walls made of bundles of thorns and brushwood.

The graduation houses consist of a timber framing, into which the faggots of thorns are built in regular walls. The structure is covered with a roof to protect it from the rain, but the sides, of course, are open to admit of the free passage of air, which, together with the solar heat, forms the evaporating medium.

The walls are from 30 to 50 feet high, and of immense length, the celebrated one at Schonebeck being fully more than an English mile in length. They are placed in the manner best suited to obtain the full benefit of the prevailing wind. The house is divided into several sections, and the weak liquor is pumped into a cistern, from which it is led by means of a perforated pipe along the top of the first division, down the sides of which it trickles into a large wooden tank underneath. From this it is pumped up and allowed to trickle through the second division, from underneath which it is pumped on to the third, and so on until it reaches the last one. In graduation houses, where the number of compartments does not exceed three, and, indeed, in all of them, to a greater or less extent, the liquor is pumped through the same division several times. The weak brine at Schonebeck contains 7½ per cent. of common salt, which, at the finish of the graduation process, is raised to about 22 per cent. In this state it is run into large tanks, of which there are eight at Schonebeck, of an aggregate capacity of about two and a half million gallons. From these tanks it is drawn off to the evaporating pans as required for boiling down. At these works the process of graduation can be carried on for an average of 250 days in the year.

The boring operations were commenced at Stassfurt on the 3d of April, 1839, and in June, 1843, had penetrated to the rock salt region. In January, 1851, when it had reached a depth of 1851 feet, the liquor from the bores contained—

Sulphate of magnesium	4·01
Chloride of magnesium	19·43
Chloride of potassium	2·24
Chloride of sodium	5·61
Total salts	31·29

However, in 1848, Professor Marchand gave it as his opinion that the salts were not mixed in the manner represented by the brine, but that pure rock salt would be found at the bottom with the more soluble salts overlying it; and so much weight was given to his opinion that in December, 1851, after having penetrated to a depth of just as many feet as there were then years in the Christian era, the sinking of the shaft "Von der Heydt" was commenced, followed in January, 1852, by that of the shaft "Von Monteuffel;" and in 1856 the pure salt was found 1066 feet from the surface.

The shaft passes through—1st, 27 feet of alluvial soil; 2d, 576 feet of sandstone, with some schist and grey limestone; 3d, 192 feet of gypsum and anhydrite; 4th, 21 feet of bituminous matter mixed with anhydrite and common salt—making in all 816 feet. Next there is 158 feet of abram or potash salts, the value of which was not recognised at first, but which now play a very important part in the industry of the country. The shaft then passes through 92 feet of rock salt, the upper portion of which is rather impure, being mixed to a considerable extent with anhydrite. This makes a total depth of 1066 feet, and at this point the lateral workings were commenced. These consist of large galleries, the principal of which are from 40 to 60 feet broad, 20 to 25 feet in height, and about 200 feet long.

The salt is wrought in a manner somewhat similar to our long wall system, a series of holes of sufficient depth, about 6 feet or so, are drilled in the face of the salt about 5 feet from the floor, and this depth of material is removed by a series of small blasts. This operation is repeated until a considerable space has been cleared under the overhanging mass of salt. Bore-holes are then drilled close to the roof, and by a series of simultaneous blasts, a large mass of salt is dislodged. In one of those halls or galleries which we visited there was lying on the floor a mass of between two and three thousand tons which had been removed in this manner a few days previously. A number of boys are employed to pick out the pieces of pure salt, which only requires grinding to fit it for do-

mestic use. The salt is removed to the pit bottom in hutches running upon rails, exactly similar to those in use in our own coal pits. From this they are lifted to the surface by an engine of 130 horse-power, and removed to the grinding-mills, of which there are twelve at the mines. There is also a 200 horse-power engine for pumping, which lifts 13 cubic feet of water per minute.

The workings into the potash salts are opened on the other side of the shaft from the common salt galleries, for although the salts are deposited one on the top of the other, still as they dip at an angle of 30°, they are all wrought from the one level.

The total thickness of the salts is 1197 feet, and this may be said to consist of—

Rock salt	989 feet.
Anhydrite	36 "
Polyhalite	13 "
Kieserite	51 "
Carnallite	98 "
Hydrated chloride of Magnesium	13 "

This gives a composition of—

Chloride of sodium	85·82
Sulphate of calcium	4·88
Sulphate of magnesium	4·70
Sulphate of potassium	0·40
Chloride of magnesium	2·53
Chloride of potassium	1·67
	100·00

—*Chemical News.*

(To be Continued.)

Lightning in a Mine.—We see in *Les Mondes* an account of a lightning flash which struck the hoisting machinery over a mine, descended the shaft for some 1800 feet, and then passed along a lateral gallery for 3600 feet more, doing various injury, by the way, before it was lost in the body of the earth. Such an action is, we believe, altogether unprecedented, and indicates a curious combination of condition by which the electric discharge could find a better conductor in the opening of the shaft and gallery than in the surrounding earth.

EDUCATIONAL

SUNLIGHT AND MOONLIGHT.

By Prof. Henry Morton, Ph. D.

(Continued from page 275.)

Passing outward from the solar photosphere or visible surface which we have described in its main features, we come to the relatively non-luminous atmosphere, by which this is surrounded to an unknown distance. In presence of the great amount of diffused light reflected from our atmosphere, we can, of course, learn little or nothing as to objects in the immediate vicinity of the sun, and it is, therefore, mainly to observations made during the rare occurrences of a total eclipse, that we owe such knowledge as we possess on this subject.*

During the continuance of "totality," that is, the time when the luminous solar disk is entirely covered by the moon, a halo is seen stretching off for an immense distance into space. This is irregularly illuminated, giving the appearance of rays, and has occasionally been observed to possess a curious shape as that of a Greek cross in the eclipse of 1860, and an hour glass in 1842. Observations with the polariscope show that the light of this halo, which is called the "corona," is polarized in planes passing through the sun's centre, which would be the case if it were sunlight reflected from particles of matter at various distances about the sun.

It is, therefore, natural to suppose that this is simply produced by the reflecting action of those scattered particles of which we have before spoken in connection with the source of the sun's light and heat.

Beside the corona there was noticed, as early as 1842, other phenomena, now known by the name of solar flames, prominences or protuberances.

These prominences consist of luminous cloud-shaped masses, of a

* The following portion of this lecture has been re-written since its delivery, on account of the very important discoveries of last August, by which the entire subject was so largely developed, and previous theories so much modified.

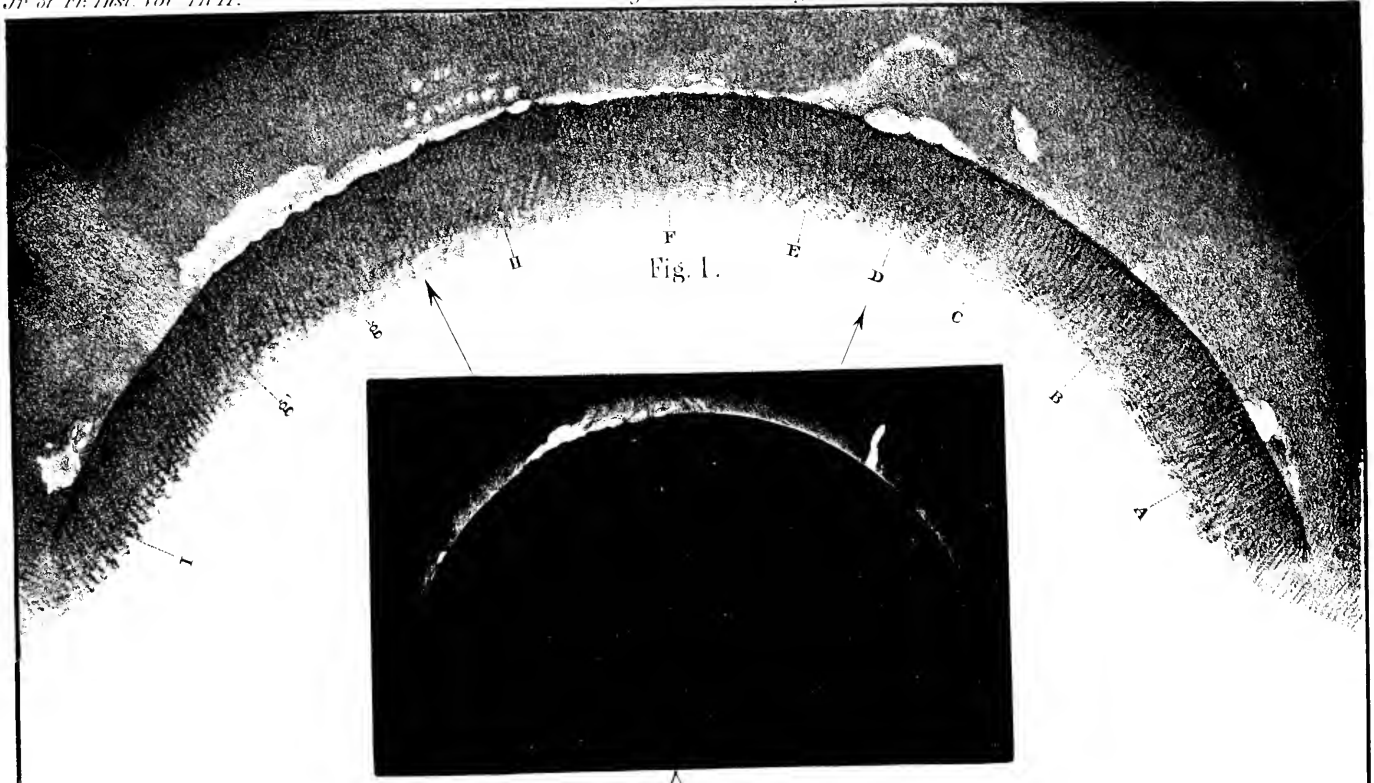


Fig. 1.

Fig. 2.

rose color, in seeming contact with, or floating near to, the solar surface. Figs. 1 and 2 in Plate VIII., will give a good idea of the general forms observed in these objects. Fig. 1 is copied from the fac-simile of an enlarged and touched photograph, published by Mr. De la Rue in the *Philosophical Transactions*, Vol. CLII., part 1.

This engraving shows but half of the original plate, and does some injustice in execution to its original, but it will give a good idea of the character of the phenomena indicated, notwithstanding these drawbacks.

It represents the prominences of the eastern limb of the sun which were by far the most interesting of those recorded on this occasion. The motion of the moon was in the direction of the arrow toward the left. The original negative from which this picture was enlarged was $\frac{3}{8}$ inch in diameter, taken at Rivabellosa, in Spain, with the Kew heliograph, which had an objective of 3·4 inches diameter and 50 inches focal length, giving a primary image of the sun 0·466 inches in diameter, which was, however, enlarged before falling on the sensitive plate by an ordinary Hygenian eyepiece, to a diameter of 3·8 inches, thus spreading the light about 64 times. The exposure given was one minute, but the negative was so intense as to present great difficulty in printing, and an observation, recorded by accident, during the exposure of the second plate, proved that fifteen seconds would have been long enough. The crooked cloud marked E, and called the "boomerang," and the scattered particles, H, were not visible to any of the observers, although clearly shown in both the photographs which were made on this occasion. (The second plate, twice displaced during the exposure, shows three good images of each.)

The other prominences were of a bright rose and violet color, and that marked C, which was entirely separate from the body of the sun, showed a conspicuous spiral structure. This fact is important in connection with other matters, as we shall presently see.

The weather at the time of this eclipse was very favorable, there being no clouds or watery haze by which either the actinic or visible rays could be arrested, in this respect making a marked contrast with the last eclipse, when at Aden the sky was so covered with clouds that two out of the six negatives exposed were entirely lost, and no trace of "non-visible" details such as were recorded on the other picture were obtained, as might be predicted, for the extreme actinic rays are exactly those which cloud and vapor would arrest.

The brightness of these prominences, under favorable conditions, may be judged from the following data. With the same instrument described above, De la Rue obtained a very faint picture of the moon in three minutes. Judging from the action on the second plate exposed during totality in 1860, an impression of the prominences equal in force, would have been obtained in one second; this would show that these prominences were 180 times as bright as the moon.

Comparing the time of exposure, on the other hand, with that needed to secure a picture of the solar disk, we find that the prominences are 696 times less bright than the solar photosphere.

From all this it would appear that with telescopes adapted to lunar photography, such as that of Mr. Rutherfurd, which makes the picture of the full moon in one second, instantaneous pictures of the prominences could readily be obtained, which would be a very desirable thing, not only because of the number of pictures which might thus be secured during the totality, but also because the errors arising from the change in relative position of the sun and moon during the exposure, could be avoided. This was the cause of some uncertainty as to the true form of several prominences, seen on Mr. De la Rue's picture, on the side nearest the lunar edge. If pictures at all equal to those made by Mr. Rutherfurd, of the moon, and which bear enlargement to a diameter of 30 inches, could be obtained, our knowledge on the subject of the solar structure would be greatly increased.

In connection with this subject of the luminosity of solar prominences, it will be interesting to notice here, that the actual darkness during total eclipse has generally been over-estimated. Thus, De la Rue says that on this occasion, the general light was brighter than the clearest moonlight, so as to admit of reading the micrometer quadrant on the eye-piece, and of drawing without the aid of artificial light.

He further described the light as resembling that of early twilight when stars of the first magnitude are visible, and others becoming so. This, however, was not his impression at the time, but was a determination reached afterwards by examining the drawings, &c., in moonlight and twilight.

The strongly marked indentations along the lunar edge in contact with the prominences, cannot be entirely attributed to actual irregularities in the lunar profile, but were due, no doubt, in most

instances, to the irregular form and brightness of the prominences, in combination with the motion of the moon, by reason of which a bright portion, impressing itself at once, would record its full extent on the negative, while the less intense parts adjacent would be unable to impress the plate until later, when the moon had moved further on, and thus cut off part of their inward edges.

Photographs taken before and after totality by an instantaneous exposure show a serrated edge to the moon, but with much smaller and less deep indentations.

Fig. 2, of Plate VIII., is a copy which in some sort, represents the most interesting of the pictures obtained last August by the North German expedition, under Dr. Vogel, at Aden.

The instrument employed was a telescope, with an objective of 6 inches diameter, and of 6 feet focus, giving an image of the sun $\frac{3}{4}$ inch in diameter. This objective was ground by Steinheil with special reference to getting the actinic rays in focus, and we may therefore be sure that had the weather proved favorable, pictures could have been taken with the shortest possible "instantaneous" exposure. The exposure actually given, was fifteen seconds, which proves (as well as the accounts of the observers and the pictures after totality, showing the clouds covering the sun) how extremely unfavorable were the atmospheric conditions.

For the various details concerning this eclipse and its observation, we would refer to the article on the subject on p. 268 of our last number. From the spectroscopic observation, made on that occasion and since, there seems little reason to doubt that these prominences are, in fact, cloud masses of very rare and intensely hot hydrogen gas, thrown out from an atmosphere of the same material surrounding the sun (to a depth of about 5000 miles) by some violent atmospheric disturbances, such as we have every reason to look for in this luminary. These prominences have, moreover, been seen to alter in shape, and even to be dispersed within a few hours, and indeed they exhibit all the characteristics of gaseous bodies.

(To be Continued.)

ON THE FUTURE DEVELOPMENT OF SCIENTIFIC EDUCATION IN AMERICA.

By S. Edward Warren, C. E.

Prof. of Descriptive Geometry, &c., in the Rensselaer Pol. Inst., Troy, N. Y.

(Continued from page 285.)

Distribution of Time.—In making a slight comparison of the following programme with others, the aggregate time allowed may seem too large. But it must be remembered that it is one thing to give a brief daily test of a student's preparation of a prescribed lesson, another thing to stand for an hour, and *impart* information to a class, not followed by any test to determine how effectually such information has been *received;* and quite another to conduct a protracted drill upon the varied applications of principles to practice. In nothing, perhaps, is French *training* more in contrast with American teaching than in the thorough drill in the *use* of principles which distinguish the former. It is one thing, for example, to read up, and fairly to understand, Analytical or Descriptive Geometry in five or six weeks, and quite another to be made perfectly at home in them, so far as pursued, by as many months' drilling in the *use* of them.

The following programme, accordingly, contemplates short and numerous lessons, and a force of instruction so large—one professor or tutor to every ten or twelve students—as to secure the utmost thoroughness of drill on practical examples in whatever is undertaken at all.

As every well settled programme of study must *grow*, and not be *made to order*, we have not taken the trouble to compare notes very diligently as to the exact proportion of time which should be devoted to the several subjects in the following scheme. The contingent of fifty hours provides for any mis-adjustments of this sort. It is only set forth as a basis, from which to proceed, till this due proportion can be determined experimentally. Moreover, different educators would differ, probably, more or less, as to what *is* such due proportion. Finally, only a programme in Constructive Engineering is laid down in full, to represent the technical courses. We have then the

GENERAL COURSE—THREE YEARS.

First Year = Third Class.

SUBJECTS.	No. Hrs. on Duty.	Remarks.
Higher Arithmetic	20	With ample practice in examples and demonstration.
Lower Algebra	70	
Elementary Geometry	50	
Mensuration and Elementary Plane Trigonometry	20	
Plane Problems, Theory and Plates	30	
Experimental Physics	60	With practice.
Elementary Geodesy	40	Field practice.
Elements of Geom., Drawing, Theory and Plates	30	
Projections, Isometrical, etc	40	
Free Drawing, Landscape patterns, etc	35	
Topographical Drawing, Elements	25	
Botany	60	With analyzed collection.
Physiology	40	
History	40	
English	55	With scientific papers.
Other Languages	85	
Contingent	50	
	750	

Second Year = Second Class.

SUBJECTS.	No. Hrs. on Duty.	Remarks.
Higher Algebra	30	With ample practice in examples.
Analytical Trigonometry	30	
Conic Sections and Higher Synthetic Geometry	20	
Analytical Geometry	45	
Higher Plane Problems, Theory and Plates	30	
Descriptive Geometry, with Theorems of Position	85	With construction of plates.
Elementary Perspective	25	
Experimental Physics	55	With practice.
Chemistry, Non-Metallic	60	
Geodesy, with Chain and Compass practice	40	
Topographical Drawing—Farm Maps	35	
Free Drawing	30	
Natural History	50	With collection of specimens.
English	30	With Elocution, etc.
Other Languages	70	
Psychology	40	
Logic	25	
Contingent	50	
	750	

Third Year = First Class.

SUBJECTS.	No. Hrs. on Duty.	Remarks.
Analytical Geometry	20	With practical examples.
The Calculus	85	
Shades and Shadows—Theory and Plates	25	
Perspective	30	
Elementary Mechanics	40	
Experimental Physics	50	With practice.
Chemistry, Metallic and Organic	70	
Geology	40	
Descriptive Astronomy	20	With telescopic examination of the Heavens.
Use of Globes	30	
Construction, etc. of Geodetical Instruments	40	
Colored Topography, Elements	30	
Free Drawing—From Nature	25	
English	45	With composition of scientific essays.
Other Languages	70	
Ethics—Æsthetics	40	
Christian Evidences	30	
Contingent	50	
	750	

Remarks (*A.*)—*Mathematics in General.*—In commenting on the composition of a course of study, designed *primarily* for promoting mental discipline, and only *remotely* for securing due professional qualifications; the use of mathematical studies, in the former relation, calls for notice, and invites an attempted answer to Hamilton's celebrated tirade against them. The main point in his argument, as we remember it, is, that as mathematics present a line of truths, each of which *necessarily* follows from the preceding, progress in them is like that of a locomotive on its track, perfectly mechanical; while, as other truth is rather *probable* than *certain*, and interminably ramified, rather than rectilinear, progress in it is more like that of a gymnast through his evolutions, and requires the best efficiency of every faculty. Let us state the case more fairly. *First:* The subject-matter of reasoning is, using Hamilton's nomenclature, of two kinds, "necessary matter" = mathematical truth, in which mathematical reasoning or reasoning on certainties is employed; and "contingent matter," in which "moral" reasoning, or reasoning on possibilities, or probabilities, is employed. Given, then, a mathematical truth. Something really does certainly, inevitably, follow from it. By reasoning, we show *how* the latter

necessarily follows from the former. Again, given a "moral" truth, as the necessary characteristics of slavery, and certain things follow, no less certainly, to the eye of Omniscience, than in mathematical truth. But, to *finite* vision, various results possibly follow. And men reason to show *how* these possible consequences may follow, or may not. In each case, the more immediate the certain or possible consequence of the given premises, the shorter and easier the reasoning process which exhibits their connection; and the more remote the consequence is, the longer and more complex the operation of demonstrating its derivation from that given truth. But the advantages of the two fields of reasoning are not so equal as this comparison might intimate. For, in the first place, it is not true that mathematical reasoning is rectilinear. It is highly ramified or divaricate, as the derivation of the equations, and, thence, all the various properties of the conic sections, from the one "general equation of the second degree," and the numerous ways of demonstrating a given proposition, may abundantly show. Grant, then, that a specific remote consequence follows from a given mathematical truth; for example, from the truth that every polynomial of the *nth* degree, which is an exact *nth* power, is formed by employing some other polynomial n times as a factor, it follows that an expression always exists, which, when so taken as a factor, will produce a given expression of that description. Now *which*, of all the more immediate truths, discovered by inspection and experiment and reflection on the structure of the polynomial, is in the line of demonstration that leads to the desired factor where in the line is it, and what follows immediately from it? To cut the matter short, will the speculating philosophers give us a rule for the solution of all equations of a higher degree than the fourth, before they write tirades against mathematics. And, meantime, will they remember, that if mathematics do "conduce to idiocy, madness and death," it may be because they so much more heavily, as well as more worthily tax the powers, than do speculations upon "being in non-being," the "reality or non-reality of time and space," the "perpetual trichotomy of thesis, antithesis, and synthesis," and the absolute Ego, the phenomenal shadow of an ultimate reality, which is our essential self."

The last topic immediately suggests another. If mathematical study consisted only in learning and applying rules, instead of also in *forming* or demonstrating rules, it would be but mechanical

thinking indeed. But, to ascertain the derivation of a rule; to translate the statement of a question, in common language, into its statement by equations; to "discuss" an equation so as to ascertain and interpret all the results which it can afford; and, not least, to transform unusual given equations into forms to which known rules will apply either at all, or most neatly—all this requires a depth and strength of thinking, and a fine and penetrating sagacity, and fertility of invention in ways of proceeding, which constitute an inimitable drill in precise and sharply definite thinking, exactly to the point.

(To be continued.)

Mr. Sandberg's paper on the Manufacture and Wear of Rails published in this *Journal*, Vol. LVI., p. 389 and Vol. LVII., p. 17, was read before the Society of Civil Engineers, London, on the 3d of March, 1868. This statement failed to appear with the paper through an accidental oversight.

Franklin Institute.

Proceedings of the Stated Monthly Meeting, Mar. 17th, 1869.

The meeting was called to order with the President, Mr. J. Vaughan Merrick, in the chair. The minutes of the last meeting were read and approved.

The Actuary submitted the minutes of the Board of Managers, and reported the donations to the Library received at the stated meeting held March 10th, inst.: From the Institute of Actuaries, the Statistical Society, the Society of Arts, London, and the Association for the Prevention of Steam Boiler Explosions, Manchester, England; l'Ecole des Mines, Paris, and la Société Industrielle, Mulhouse, France; the Water Commissioners, Detroit, Michigan; the Managers of the State Lunatic Asylum, Utica, New York, and Dr. T. S. Kirkbride and Dr. G. Emerson, of Philadelphia. Also, that, on the report of a Special Committee appointed to consider the advisability of, and a plan for a sectional arrangement of the Franklin Institute, the Board of Managers had adopted Regulations for the oganization and Government of Sections, which were therewith submitted.

In accordance with a previous resolution of the Institute, the following memorial was then read by Prof. Fairman Rogers:

BIOGRAPHICAL NOTICE OF PROF. ALEXANDER DALLAS BACHE.

Alexander Dallas Bache, the subject of our memoir, was born in Philadelphia, on the 19th of July, 1806. His father, Richard Bache, was a son of Richard Bache, Postmaster General of the United States, and Sarah, only daughter of Benjamin Franklin. His mother, Sophia Dallas, was the daughter of Alexander James Dallas, Secretary of the Treasury, and the sister of George M. Dallas, Vice President of the United States and Minister to London.

Mr. Bache was thus connected with families alike distinguished for scientific and intellectual attainments and social position, in his native city.

In 1821 he entered the United States Military Academy at West Point, and graduated at the head of an uncommonly good class in 1825, remaining for about a year after his graduation as assistant instructor in engineering. He was then assigned to engineer duty at Fort Adams, Newport, R. I., where he remained two years, with General, then Colonel Totten, and while there became engaged to Miss Fowler, to whom he was married in 1828, a woman adorned with those good qualities which enabled her to be his firmest friend and most able adviser throughout his life. In 1828 Mr. Bache was appointed Professor of Natural Philosophy and Chemistry in the University of Pennsylvania, at the age of twenty-two, and it is from that time that his scientific career commences. In 1836 he was appointed the first President of Girard College, and made his trip through Europe in the interests of that institution. Upon his return, finding that much time must yet elapse before the College buildings could be finished, he offered his services to the city to re-organize its High School, and at the end of a year, the College not yet being in a condition to require his attention, he resigned his salary, but retained the office of President, holding himself ready for duty when the building should be in readiness, and became Principal of the High School and Superintendent of Public Schools, for which he received a salary from the city. He again took his old chair in the University in 1842, and in 1843 he was appointed Superintendent of the United States Coast Survey, which position he held until his death, which occurred on the 17th of February, 1867.

Such is a brief record of one of the most useful and brilliant lives of the past half century. His devotion to science, in the highest

acceptation of the term, knew no bounds. His enthusiasm not only carried him forward, but it communicated itself to all who came in contact with him. One reason that he did so much work, was that he was hundred-handed. The moment that he made the acquaintance of a new man he saw what he was good for, what he could do, and, by some mysterious power, he set him to work. When he proposed a line of research, or a matter to be worked up, it always seemed to him to whom he proposed it, that here was just the opportunity for which he had been waiting, and he attacked it with vigor and determination. Thus the great chief always had many hands and brains occupied with the details of the matters which were at the time interesting him, and these outside labors met with so much consideration, and were so handsomely acknowledged, that no one hesitated to repeat them when called upon a second time. No scientific man could render a greater service to science than by encouraging its younger votaries to take up and to persevere in those investigations for which nature may have fitted them, and in this Mr. Bache was eminently successful. He showed this conspicuously when, even as a young man, he was interested in the operations of this Institute, and it is in his labors as one of our old members that we are especially interested.

Very shortly after his appointment to the University, Mr. Bache connected himself with the Franklin Institute. On the 25th of March, 1830, his name first appears in the records of the Society, as chairman of the monthly meeting of that date, and from that time until his departure for Washington, in 1844, he was a prominent member, serving faithfully on most of the important committees. He was in the Board of Managers from 1831 to 1839, and Corresponding Secretary from that time until 1843. He served for many years on the Committees on Meteorology, Inventions, Instruction, Meetings, and Publication, and on the Committees on Patent Laws and the Manufacturing Establishments of Pennsylvania. The Committee on Inventions was extended into the Committee of Science and the Arts in 1834.

He was appointed on the Committee on Explosions of Steam Boilers and Strength of Materials, in June, 1830, and labored faithfully as a member of both its sub-committees, taking a large part in the preparation of the report, which is still considered one of the classical authorities on the subject. He also took a large share in the labors of the Committee on Experiments on Water Power. As a member of the Committee on Weights and Measures, in 1833, he

took a large share in the preparation of the valuable report on that subject. In October of 1842 he delivered the address at the close of the Annual Exhibition. During all this time he contributed many papers to the *Journal* and to the Philosophical Society, of which he was an active member. He edited an American edition of Brewster's Optics, and conducted a large number of investigations in Magnetics, Meteorology, and Physical Science generally. This was probably the time of the greatest activity of the Institute, and Mr. Bache had associated with him as fellow-workers, and as staunch friends for all after life, S. V. Merrick, Frederick Fraley, Dr. Hare, M. W. Baldwin, T. U. Walter, Sears C. Walker, J. C. Cresson, J. F. Frazer, men whose names we now honor as among the brightest lights of the Society.

In 1843, Mr. Bache having received the appointment of Superintendent of the United States Coast Survey, left Philadelphia, and, of course, his active duties in the Institute, to take up his residence in Washington, but he always considered this city as his home, and looked forward to returning to it at some future period, when his labors for the Government should be ended; and one of his greatest pleasures was to meet his old associates on the occasions of his frequent visits to Philadelphia, and talk over the times when they worked together for the interests of the Institute and of science.

When Professor Bache took hold of the Survey, he found himself in a position which required all his tact to make comfortably tenable. Some of the older assistants felt aggrieved that a person hitherto unconnected with the work should have been selected as its head, and for many months there was a disposition to make things go roughly, which might have disheartened a man who had smaller views, or less disposition to seize upon the opportunity afforded him to make his new work one of the grandest contributions to the science of the age.

Extreme firmness, imperturbable good humor, and a manner which made all who approached him friends and totally disarmed his enemies, finally enabled him to overcome all obstacles in the interior of the survey, and he applied himself with all his energy to the elaboration of the organization, and the introduction of all the best scientific methods, most of which he extended in their practical application, to a point not before reached.

He soon enlisted the best scientific power of the country, either as officers of the Survey, as temporary assistants for some special

work, or as friends, who, for pure love of the man and interest in a work to which he devoted his energies, were always ready to contribute their advice or co-operation in those matters which belonged specially to their line of study. It was in this way that he won the title of "Chief," applied to him by a large and ever increasing circle of scientific men, who appreciated him as the leader of organized science in America.

His peculiar position gave him advantages which could hardly be enjoyed by any other scientific man. Visiting each seaboard city frequently in the discharge of his official duties, he was constantly in personal contact with his acquaintances, and had every opportunity of seeing the new men who grew up in each place. Accustomed to the details of commercial and political business, he had much broader and more practical views than those which are sometimes the result of seclusion in the study or the laboratory. The fact of his having graduated with all the honors of the Military Academy placed him upon a footing with the officers of the army and navy, which was of the greatest advantage to him in his connection with both branches of the service. It has always been usual to detail some army and navy officers for duty upon the Survey, and it not unfrequently happened that Bache had to administer one of his quiet reproofs to some young officer, who, forgetting, or perhaps ignorant of the fact, that his Chief was a regular army man, would attempt to plead a "custom of the service," or a point of etiquette, as an objection to some distasteful duty. Of that rare power of administration which appears to be partly natural and partly the result of education, Mr. Bache had a large share, and the scientific and business operations of the Survey moved like clock-work under his guiding hand. He loved to put the machinery together, wind it up, and then dismissing it all from his mind, hear the report at the end of the month or designated time, when he would take up the thread of the matter just where he had last left it, as if he had thought of nothing else during the interval. He understood thoroughly the way of doing nothing for himself that could be done for him by others, and thus reserved his time and his powers for that work which he alone could do. His practical knowledge of methods of observation was extrordinary, and he could pick out interpolated figures in records of work, or tell an astonished observer that on such a night he had omitted to examine the level of his instrument, with an accuracy that bordered upon the marvellous.

His capacity for work was astonishing. Not contented with the large and ever increasing labors of the Survey, he was an active member of the Board of Regents of the Smithsonian Institution; associated there with his warmest and most trusty friend, Professor Joseph Henry, of the Light House Board, of numerous special boards on harbor improvements, President of the National Academy of Sciences, and ready at all times, and constantly called upon, to use his tongue or his pen to advance the true interests of science at all points.

It is not common that with those abilities to which we have referred are combined those social qualities which render their possessor agreeable in ordinary life, but Mr. Bache possessed them in an eminent degree. Released from his official duties, about which he was usually very methodical, he was the pleasantest companion at the dinner table or in the saloon, that young or old could desire. Extremely fond of society, his hospitable house in Washington was always open to his friends, who carried away with them the most charming reminiscences of its bright wood fires and sparkling candles, and in his summer camps there were always some extra tents for those who were fortunate enough to receive invitations to visit him in his wild retreats. He spent several months of each year under canvas, at the primary triangulation stations, or on base measurement, and returned to his duties in the capital refreshed and invigorated by the mountain air, long strolls, and change of scene. Bright reminiscences are those of these mountain camps, with the morning's writing, the midday dinner, the genial face of the kind hostess, the pleasant chat over the bottle of Rhine wine, and, if there was no observing in the afternoon, the long rambles down the hill, with the climb back again, the camp being of necessity very near to the summit, finishing up with an evening of conversation or reading, unless the stars were good enough to allow themselves to be observed.

With a never flagging determination to carry the scientific operations of the Survey up to and beyond the highest point of excellence attained in other countries, Mr. Bache spared no pains or thought in perfecting all the details of the various processes with such success that in every branch important steps were made. To the apparatus for the measurement of bases especially, as being the instrument upon which the accuracy of succeeding work depended, he early turned his attention, and produced a base measurer which

is yet without its equal in the world. In 1845 he took up the subject, and discarding the principle of using surface marks or dots on the measuring bars, and bringing them into coincidence by means of microscopes, he applied the contact level already invented by Repsold for another purpose, and by thus introducing the method of end contact, facilitated the comparisons with the standard bars, the practical working in the field, and the accuracy of the operations in a remarkable degree. The extremely beautiful method of equalizing the conducting power and consequent rapidity of expansion of the iron and brass bars of the apparatus, by making their sections proportional to their conducting powers and specific heats, and then making the final and most delicate correction by applying varnishes of different colors, is an admirable example of the care which he bestowed upon the smallest details.

Having found the metrical system in use on the Survey as introduced by Mr. Hassler, he continued it, and always felt a deep interest in the adoption of that, or of some other universal system of weights and measures, by the civilized world. His position as Superintendent of Weights and Measures, of course, brought the subject constantly under his notice. As a member of the Committee on Weights and Measures of the National Academy, he also discussed the matter thoroughly, and at one time he leaned evidently towards making an attempt to establish, by a Congress of nations, an *entirely new* standard, which should be adopted as an universal one. His two general objections to the metrical system were: first, the fact that later observations have shown that the metre is not the 10,000,000th part of the earth's quadrant; and secondly, that the actual length of the metre is not, in practice, nearly so convenient as that of the foot or the ell. The latter is no doubt the strongest objection that can be made to the metre as an universal standard, and perhaps the only one; and later, Mr. Bache seems to have determined that the metric system had too strong a hold to be rooted out by any other where it already has been adopted, and was prepared to give his unqualified support to any measures looking to its adoption as an universal system. It is a little singular that the period of his death witnessed the legalization, to a limited extent, of the metric system in England and the United States, and the consummation of one of his most cherished projects, the determination, by the Atlantic Telegraph Cable, of the difference of longitude between Greenwich and Washington, which was

made for the Coast Survey in December of 1866, by Dr. B. A. Gould. For several years, in fact since the laying of the original cable, everything had been prepared for these observations, and the preliminary report of their success was made to the National Academy of Sciences in January, but one month before his death.

During the rebellion Mr. Bache threw himself, heart and soul, into the service of the Government, and took a most arduous part in the labors of the blockade commission, at the very outset of the war. His judgment and far-sightedness enabled him to withdraw his parties and vessels from the South almost without a loss, and no information of any value fell into the hands of the rebels from Coast Survey sources. The Survey furnished most valuable officers during the war for military surveys, and they were much needed, since the engineer officers, being all graduates, were rapidly promoted to line appointments, and technical knowledge in that branch of the staff was sadly needed.

In 1861 Mr. Bache became Vice-President of the Sanitary Commission, and was throughout the war a most influential member of that imporfant body. When Lee threatened Philadelphia in the Gettysburg summer, Mr. Bache did not forget his native city, but immediately offered his services, and those of the officers he had near him, to make a military reconnoissance of the vicinity of the city, and to locate works, to be built if the necessity required. In that sultry summer weather he worked, literally, day and night, and exhausted a frame never much accustomed to severe bodily exercise, and it is doubtful whether he ever recovered entirely from the effects of the labor and worry which he underwent at that period.

When the National Academy of Sciences was established by Congress, in 1863, the choice for President fell, without dissent, upon Mr. Bache, and he continued to discharge the duties of the office with the greatest energy and judgment until his illness withdrew him from active labors. His guiding hand, his moderate counsels, and his constant vigilance in seizing upon every turn which could be of advantage to the scientific usefulness of the Academy, were of the utmost importance to its welfare and power. In the spring of 1864 his health began to fail him. Too much intellectual labor had done its work, the body was too weak for the mind, and a long summer rest, and finally a trip to Europe, were prescribed as the cure. Temporary relief, however, was all that these

means afforded, and he spent the remainder of his life in Newport, where he had commenced his public career after leaving the Military Academy.

He died in Newport, on the 17th of February, 1867, and was buried at Washington, in the Congressional Cemetery, on Sunday, the 24th. The honors paid to his remains, as they passed through New York, Philadelphia and Baltimore, were a fitting tribute to his virtues. He was laid in state, in the old hall of the Philosophical Society, which, in the early days of his Philadelphia career, had known him so well, and all his friends who knew him in life, and many who, knowing him only through his works, loved him almost as well, crowded around the bier to pay their last tribute of respect to his memory.

It is rare that a man leaves us without a successor appearing to take his place, but the void which Mr. Bache leaves in the general scientific strength of the country has not yet been filled.

The committee appointed to make application to the Legislature for the use of one of the Penn Squares, reported that they had presented a Memorial asking for authority to occupy the southeastern angle of the above named Square for the purpose specified.

The Report of the Resident Secretary on Novelties in Science and the Mechanic Arts was then read, after which the following preamble and resolutions were unanimously adopted:

Whereas, A bill has been presented to the Legislature of Pennsylvania whereby steam users will be compelled to apply various patented attachments to their boilers; and *whereas*, the proposed legislation will not be as efficient in preventing accidents from the use of steam boilers as the law now in existence within the limits of the city of Philadelphia; therefore,

Resolved, That the Franklin Institute respectfully remonstrates against any such legislation, believing that it will impose on steam users an onerous tax, without accomplishing any good result.

Resolved, That the Secretary be, and he is hereby instructed, to forward copies of the above preamble and resolution to the Governor of this Commonwealth, and to the Speakers of the Senate and House of Representatives.

The meeting was then, on motion, adjourned.

HENRY MORTON, *Secretary.*

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OF THE STATE OF PENNSYLVANIA,

FOR THE

PROMOTION OF THE MECHANIC ARTS.

VOL. LVII.] JUNE, 1869. [No. 6

EDITORIAL.

ITEMS AND NOVELTIES.

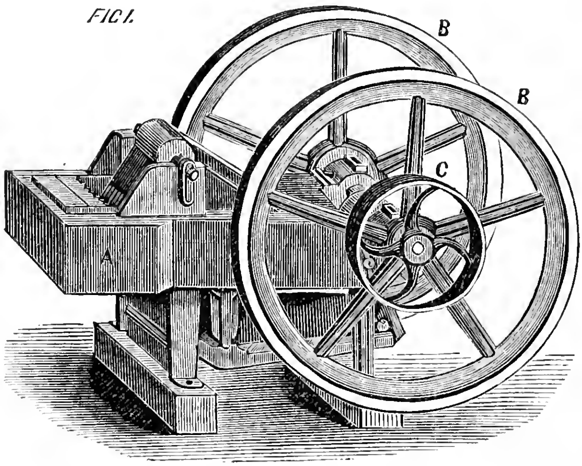
Blake's Stone-Breaker.—The large quantity of broken stone required for the construction of Macadamized roads, as well as for ballasting for railroad tracks, calls for the employment of machinery for crushing or breaking stone. Machines for this purpose have been successfully used for several years, both here and abroad.

The attempt has been made in England to use machinery somewhat like the coal-breakers employed at our anthracite mines for crushing rock. One such machine, at a large quarry, was erected at a cost of several thousand pounds sterling, and did its work very effectually. But stone crushed at one quarry can only be transported economically for road-making to a very limited distance. Stone found at various points on any road should be broken as near the place where it is to be used as possible: hence, portable stone-breakers are more economical in use. There are several such

machines now in operation, but the success of one in particular induces us to present it to our readers. This machine, called "Blake's Stone-Breaker," shown by the annexed cuts, is designed to break stones into small fragments, to be used for road-making, railroad ballasting, preparing concrete or other purposes, and to crush ores or minerals of any kind. It is simple and compact, and, being complete in itself, requires no extraneous support or fixtures.

Fig. 1 is a perspective view of the machine entire. The frame, A, which receives and supports all the other parts, is cast in one piece, with feet to stand upon the floor or on timbers. These feet are provided with holes for bolts, by which it may be fastened down

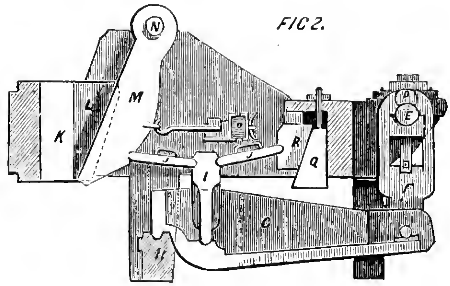
Fig. 1.



if desired; but this is unnecessary, as its own weight gives it all the stability it requires. B B are fly-wheels on a shaft which has its bearings on the frame, and which, between these bearings, is formed into a short crank. C is a pulley on the same shaft, to receive a belt from a steam engine or other driver.

Fig. 2 shows a side-view or elevation of the other parts of the machine in place, as they would be presented to view by removing one side of the frame. The parts of this figure which are shaded by diagonal lines, are sections of those parts of the frame which connect its two sides, and which are supposed to be cut asunder, in order to remove one side and present the other parts to view. The dotted circle, D, is a section of the fly-wheel shaft, and the circle, E, is a section of the crank. F is a Pitman or connecting

rod which connects the crank with the lever, G. This lever has its fulcrum on the frame at H. A vertical piece, I, stands upon the lever, against the top of which piece the toggles, J J, have their bearings, forming an elbow or toggle-joint. K is the fixed jaw, against which the stones are crushed. This is bedded in zinc against the end of the frame, and held back to its place by cheeks, L, that fit in recesses in the interior of the frame on each side. M is the movable jaw.



This is supported by the round bar of iron, N, which passes freely through it and forms the pivot upon which it vibrates. O is a spring of india-rubber, which is compressed by the forward movement of the jaw and aids its return.

Every revolution of the crank causes the lower end of the movable jaw to advance towards the fixed jaw about $\frac{1}{4}$ of an inch and return. Hence, if a stone be dropped in between the convergent faces of the jaws, it will be broken by the next succeeding bite; the resulting fragments will then fall lower down and be broken again, and so on until they are made small enough to pass out at the bottom. The readiness with which the hardest stones yield at once to the influence of this gentle and quiet movement and melt down into small fragments, surprises and astonishes every one who witnesses the operation of the machine.

It will be seen that the distance between the jaws at the bottom limits the size of the fragments. This distance, and, consequently, the size of the fragments, may be regulated at pleasure. A variation to the extent of $\frac{5}{8}$ of an inch may be made by turning the screw-nut, P, which raises or lowers the wedge, Q, and moves the toggle-block, R, forward or back. Further variations may be made by substituting for the toggles, J J, or either of them, others that are longer or shorter; extra toggles, of different lengths, being furnished for this purpose.

This machine may be made of any size. Each size will break any stone, one end of which can be entered into the opening between the jaws at the top. The size of the machine is designated by the size of this opening; thus, if the width of the jaws be 10 inches,

and the distance between them at the top 5 inches, the size is called 10 by 5.

The product of these machines per hour, in cubic yards of fragments, will vary considerably with the character of the stone broken. Stone that is *granular* in its fracture, like granite and most kinds of sandstone, will pass through more rapidly than that which is more compact. The kind of stone being the same, the product per hour will be in proportion to the width of the jaws, the distance between them at the bottom, and the speed. The proper speed is about 180 revolutions per minute; and, to make good road metal from hard, compact stone, the jaws should be set from $1\frac{1}{4}$ to $1\frac{1}{2}$ inches apart at the bottom. For softer and for granular stones, they may be set wider.

The following table shows the several sizes of machines now in use; the product per hour of each size of fine road metal from the hardest materials, when run with a speed of 180, the power required to perform this duty; the whole weight of each size, in round numbers, and the weight of the heaviest piece when separated for transportation.

Size.	Product per hour.	Power required.	Total Weight.	Weight of frame and parts attached.
10 × 5	4 cubic yards.	6 horse.	6,600 lbs.	3,200 lbs.
10 × 7	4 " "	6 "	7,600 "	4,100 "
15 × 5	6 " "	9 "	9,100 "	4,700 "
15 × 7	6 " "	9 "	10,200 "	5,600 "
15 × 9	6 " "	9 "	11,600 "	6,800 "

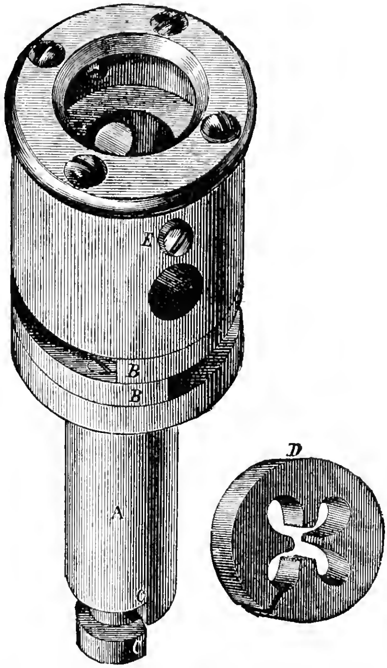
The whole length of the machine to the back of the wheels is from 8 to $8\frac{1}{2}$ feet; height to top of wheels, 5 feet; width, from 4 to 5 feet.

So far as Macadamized roads are concerned, the experiments tried in Central Park, New York, and described in the paper by Mr. W. H. Grant, C.E., (see this *Journal*, Vol. LIII., p. 11, *et seq.*) as well as the constant usage of the road-makers, in France especially, shows the necessity of employing on the freshly-laid layers of metal or crushed stone, heavy rollers for compacting. On American roads it has been the custom to place the stone, and trust to its being com-

pacted by the wagons going over it. Any one who has watched this process of road-making cannot fail to see its disadvantages. The bare broken stones are hard on the horses feet, and the wagon-wheels soon form tracks, which, if not attended to by filling up, grow into deep ruts.

We learn that the Commissioners of Fairmount Park have imported a steam rolling machine for this purpose; and as they have already began to lay out and make the new roads in the recent addition to the Park, we hope at an early day to lay before our readers a detailed account of the operation of the machine.

Die Holder for Screw Machines, by Brown & Sharpe, of Providence, R. I. In threading screws and in tapping, it is often desirable to cut the thread up to a shoulder or to a given distance and positively no further, and this is necessarily quite a difficult and delicate operation, requiring great expertness to avoid breaking the threading tool or injuring the shoulder of the screw. The accompanying cut represents a device to overcome this difficulty. The die holder there shown consists of two parts, one sliding within the other. The small end, A, of the outer piece is inserted in one of the holes of the revolving head of a screw machine, and is fastened there by the two screws.



The inner piece, in which the die is held, is free to move endwise, but is kept from turning round in one direction by two jaws, B B, one projecting from the end of the inner, and the other from the outer piece, which lock the two parts together. When the die-holder is in use in the machine, the revolving head is kept moving forward till it is stopped by the screw, L, when the inner part holding the die, is drawn forward by the screw which is being cut, till it unlocks from the outer part,

when the forward motion of the die instantly ceases, and it merely revolves.

The motion of the spindle is then reversed, and the revolving head moved back by the lever or hand-wheel, which locks the two parts of the holder by another clutch, C C, at the small end, the jaws of which are in a reversed position to those before mentioned, and the screw turns out of the die. The large end of the holder has a recess for the die, D, which is held in its place by a cap screwed over the outer end. The die used is split on one side and two screws, one of which is shown at E, are inserted in the holder to close it up as wear takes place. In tapping, the large end of the inner part of the holder is made with a socket to raise a tap which is held in its place by a set screw.

Bridge with Warren Girders on the Pennsylvania R. R.—There has just been erected a Warren girder bridge, single system, with vertical carrying-rods, over the Juniata river, east of Tyrone Station, on the main line of the Pennsylvania Railroad.

The structure, which is entirely of iron, even to the floor-beams, contains some details of construction which may prove of interest to the engineer and bridge-builder, and serve to show one of the latest American bridges with the Warren form of bracing.

The main features of the construction may be stated in a few words. Wrought iron upper flange, composed of deck-beams with rolled plates riveted on top and reinforced towards the centre of the span by the addition of other plates, riveted either to the top or the inner side of the deck-beams. Also, with the web of the deck-beams thickened at the pin-holes to give the required bearing surface for the pin. All the parts being so arranged as to allow the whole surface of the chord, inside and out, which may be liable to rust, to be easily examined, and painted from time to time. Lower flange, composed of wrought iron rolled links, with upset head and eye at each end. Main bracing also made of wrought iron links, with eyes and upset head where required, bulged at the centre of each member, at right angles to the plane of the truss, and stiffened by rivets and distance-sleeves. The inclined end-post is of cast iron, and the whole truss is held together by connecting-pins, carefully proportioned, at the intersection of the members.

The ends of these connecting-pins, in the lower flange, are planed down to a thickness, and have the wrought iron lateral strut and bracing bolted directly to them. This arrangement, while it keeps

the lateral bracing entirely independent of the floor-beams or road-way, braces the truss at the most effective point by a very simple and satisfactory method. Its applicability, also, to roof-trusses and various other constructions in which connecting-pins of either medium or large size are used, is at once apparent.

Another deck-bridge, for double track, of three spans of eighty feet each, constructed with double system of triangles braced at their intersections, but with the same general details as this bridge, has just been erected on the main line of the Pennsylvania Railroad over the Conemaugh river, near Summerhill Station. When tested it showed very satisfactory results; and the object aimed at when preparing the design seems to have been accomplished, viz: to obtain, without additional cost over that incurred heretofore for iron bridges in similar positions, a self-adjustable truss which will require no screwing up after being once placed in position, and, at the same time, having the details so arranged that they can be easily examined, and the truss painted in every part, from time to time.

The designs for these bridges were made in the Principal Assistant Engineer's Office of the Construction Depot of the Pennsylvania Railroad, and the work executed by the Keystone Bridge Company, Pittsburg, Pa.

Pneumatic Piles—Rapid Work.—In a letter just received from Mr. Robert Cartwright, at the Boomer Bridge Works, Chicago, he tells us that they are now casting pneumatic piles for the Omaha and Leavenworth bridges, in sections ten feet long by eight feet six inches in diameter, and with one and three-quarter inches thickness of metal. An iron core barrel is employed, and all the castings thus far made have been perfect. They expect next week to turn out these sections at the rate of one per day. On the occasion of the "last rail celebration," last Monday, one of the pile sections weighing nine and a half tons, and on a truck drawn by sixteen horses, was added to the procession. It was not thought of until 10 A. M., and was then being drilled at the works four miles out of the city, but by one o'clock it was in its place in the procession.

Chicago Street Tunnels.—At a late meeting of the City Councils, the following communication from the Board of Public Works was referred to the Committee on Harbor and Bridges:

"The Board of Public Works submit herewith a copy of the report of the City Engineer, with estimates of cost for tunnels under the river at La Salle Street and Adams Street. The Board also

present with this report various drawings, exhibiting in detail the tunnel as proposed at La Salle Street, for the information and inspection of the Council. It is the desire of the Board, however, that these drawings may be returned to this office when no longer needed by your honorable body, as the preparation of a second set would require much time and expense. A general estimate of expense shows that the La Salle Street tunnel will cost about $500,000, and the Adams Street tunnel about $400,000. The Board submit with this report an ordinance authorizing the construction, and providing for the cost of each of the two tunnels; but it is the recommendation of the Board that but one of the two be undertaken at a time."

There was also presented the following communication from Mr. Chesbrough:—

Chicago, May 12, 1869.

Board of Public Works.

Gentlemen:—Drawings and estimates of a proposed river tunnel, between the South and North Divisions on La Salle Street, and an approximate estimate of one between the South and West Divisions on Adams Street, are herewith respectfully submitted.

As will be perceived by examination, the plan proposed for La Salle Street is, in its main features, very similar to that of the tunnel already constructed on Washington Street. Its total length, including open approaches, is 1,930 feet, extending from the north side of Randolph Street to the south side of Michigan Street. The grade on the south side of the river is 1 in 22·57, and on the north side 1 in 18·80, but the latter could be reduced to 1 in $20\frac{7}{8}$ by a slight alteration of the grade of Michigan east and west of La Salle Street a short distance north.

In order to obtain uniform grades on each side of the river it will be necessary to carry Lake and Kinzie streets across the tunnel on iron girders filled in between with brick arches, which are to be covered over with water tight roofing, and thick planking laid across the girders. This mode of construction would give sufficient head room for carriages of all kinds under those streets. As soon as the depth of covering becomes sufficient to admit of arches of masonry these are adopted.

The length of covered masonry tunnel is 1,141 feet, and the length on both sides of iron girder covering is 195 feet, making a total length of covered carriage-way of 1,336 feet. The entire

length of the foot-way is a little over 1,000 feet. It is proposed to put this on the east side of the street.

The total rise from under the centre of the river to the grade of the street each way is forty-two and a half feet.

There has not been sufficient time yet, in consequence of pressing duties, to complete specifications of the work, but it is proposed, in order to avoid some of the difficulties that occurred in the construction of the Washington Street River tunnel, to have the excavation for this made in short sections at a time, and the masonry immediately built in, so as to prevent, if possible, any damage to buildings along the work. It is also proposed to require the contractors to roof over the river portions of the tunnel under construction in the winter, to preserve in it at all times a temperature above freezing, and to carry on this portion of the work night and day both winter and summer.

Although the leakage through the roof and sides of the Washington Street River Tunnel is rapidly diminishing, and will probably cease to give any annoyance, whatever, before the end of summer, it is thought best, in order to avoid any temporary inconvenience from this source, to provide such drainage in the centres of the arches, abutments and piers under the river, as will divert all unavoidable leakage directly to the sewerage under the tunnel.

Experience with the Washington Street River Tunnel leads to the belief that concrete under the masonry may safely be dispensed with. This has accordingly been omitted in the plans and estimates now presented.

The estimated cost of this work is $457,342 32. This estimate includes ten per cent. for contingencies, but nothing for alteration of sewers, water or gas pipes. It would probably be more prudent to call the total sum $500,000, in view of the possibility of claims of various kinds for damages, and of occurrences difficult to foresee in a work of this kind.

The plan proposed for the Adams Street tunnel, with regard to length, grades, width and form of road and footways, is very much the same as that on Washington Street. The same suggestions as those made for the La Salle Street tunnel relative to omitting concrete under the foundations, drainage under the river, and carrying on the river portion of the work night and day, during winter and summer, are recommended for this.

The total length of the Adams Street tunnel, including open ap-

proaches, is 1,527 feet, extending from the west side of Franklin Street to the east side of Clinton. The length of covered carriage-way is 934 feet. The length of foot-way about 820 feet. The grade on the east side is 1 in 16, and on the west side 1 in 1863. The total ascent from the lowest point under the middle of the river to the centre of Franklin Street is $42\frac{1}{2}$ feet, and on the west side to the centre of Clinton Street is $43\frac{3}{4}$ feet.

The total estimated cost has not been quite so thoroughly made yet as that for the La Salle Street tunnel, but, including contingencies, it may be set down approximately at $400,000.

Much consideration has been given to the subject of constructing the river portions of the tunnels of iron, it having been supposed that not only the obstruction to navigation caused by coffer dams could be avoided, but considerable expense be saved besides. It has also been supposed that an iron tunnel could be made much tighter than one of masonry.

No information your engineer could obtain in the East or elsewhere, shows that any work of that kind has ever been successfully carried out in any part of the world, although one has been commenced in London; but there is no reason to doubt the practicability of it. A number of plans have been proposed during the past fifteen years for constructing tubular tunnels in this country and in Europe, but in nearly, if not quite every case, by persons who evidently were not aware of the practical difficulties they had to encounter. The wonderful progress of the last few years in this branch of engineering, makes it more prudent now than it was formerly to attempt such works. Two different parties, composed of men of the highest skill and practical experience, and possessed of ample means to carry out such works as these, are desirous of being permitted to submit plans and propositions for constructing one or both of them. They have been encouraged to believe that whenever the board may be ready to receive proposals for the construction of one of these tunnels, their propositions would be carefully considered. When such propositions are received, it will be much easier than now to state what probable advantages, if any, that mode of construction would possess over the one which, according to all light and experience of the present, it seems most prudent to adopt.

The work of preparing these plans and estimates has been mostly

performed by Mr. William Bryson, whose past experience in works of this kind makes his services very valuable.

Respectfully submitted,

E. S. CHESBROUGH.

Sugar and Starch from Sweet Potatoes.—At the last meeting of the Institute, there were presented by Mr. Henry Bartol, of this city, samples of sugar and starch, with the following analysis made November 6th, 1869, from juice of North Carolina sweet potatoes.

	PER CT.
Sugar	10·50
Starch	6·00
Various salts	1·17
Gum, etc.	·33
Water	82·00
	$100·00

By the use of powerful presses, 90 per cent. of the weight of the potato can be obtained in juice; the density of this juice is 10·5° Beaume, and according to the formula used in Europe for determining the probable yield of the beet, these potatoes would yield 6·18 per cent. of sugar, and I have no doubt that 5 per cent. of starch could be extracted. Nine tons is considered a fair yield per acre. When the cultivation of the beet was commenced as a sugar growing plant, it contained only 4 to 5 per cent. of sugar. To-day, by improved culture, it contains on an average, 12 per cent. With this example before us, what might we not hope to accomplish with the sweet potato as a source of sugar?

Sun-Spots, undoubtedly Depressions.—In an abstract of some recent observations on sun-spots, made at the Kew Observatory, by Warren De La Rue, Esq., F.R.S., Balfour Stewart, Esq., F.R.S., and Benjamin Loewy, Esq., F.R.A.S., we find the following abstract:

The authors, after reviewing briefly the two theories on the nature of sun-spots, which are still subjects of dispute, refer to the stereoscopic views obtained and the results published in their "Researches on Solar Physics," and state the reasons which have led them to believe that sun-spots are cavities and at a lower level than the sun's photosphere. Their opinion has been recently strengthened by observations of a sun-spot on the 7th of May, which in disappearing produced in two successive photograms indentations in the west limb.

After proving by the measurements made, which, with the calculations, are appended to their paper, that there can be no doubt about the identity of the heliographical elements of the previously observed spot and the successive indentations, they prove from the established details of the phenomena of sun-spots that such indentations must under all circumstances be very rare occurrences, and state fully the conditions favorable to the recurrence of similar observations, inviting observers to give their particular attention to them.

Why a Bell will not ring in Hydrogen Gas.—In the abstract of a paper by Professor G. G. Stokes, we find the following:—

" In the first volume of the Transactions of the Cambridge Philosophical Society will be found a paper by the late Professor John Leslie, describing some curious experiments which show the singular incapacity of hydrogen either pure or mixed with air, for receiving and conveying vibrations from a bell rung in the gas. The facts elicited by these experiments seem not hitherto to have received a satisfactory explanation.

" It occurred to the author of the present paper that they admitted of a ready explanation as a consequence of the high velocity of propagation of sound in hydrogen gas operating in a peculiar way. When a body is slowly moved to and fro in any gas, the gas behaves almost exactly like an incompressible fluid, and there is merely a local reciprocating motion of the gas from the anterior to the posterior region, and back again the opposite phase of the body's motion, in which the region that had been anterior becomes posterior. If the rate of alternation of the body's motion be taken greater and greater, or, in other words, the periodic time less and less, the condensation and rarefaction of the gas, which in the first instance was utterly insensible, presently becomes sensible, and sound-waves (or waves of the same nature in case the periodic time be beyond the limits of audibility) are produced, and exist along with the local reciprocating flow. As the periodic time is diminished, more and more of the encroachment of the vibrating body on the gas goes to produce a true sound-wave, less and less a mere local reciprocating flow. For a given periodic time, and given size, form, and mode of vibration of the vibrating body, the gas behaves so much the more nearly like an incompressible fluid as the velocity of propagation of sound in it is greater; and on this account the intensity of the sonorous vibrations excited in air as compared with hydro-

gen may be vastly greater than corresponds merely with the difference of density of the two gases.

"It is only for a few simple geometrical forms of the vibrating body that the solution of the problem of determining the motion produced in the gas can actually be effected. The author has given the solution in the two cases of a vibrating sphere and of an infinite cylinder, the motion in the latter case being supposed to take place in two dimensions. The former is taken as the representative of a bell; the latter is applied to the case of a vibrating string or wire. In the case of the sphere, the numerical results amply establish the adequacy of the cause here considered to account for the results obtained by Leslie. In the case of the cylinder they give an exalted idea of the necessity of sounding-boards in stringed instruments; and the theory is further applied to the explanation of one or two interesting phenomena."

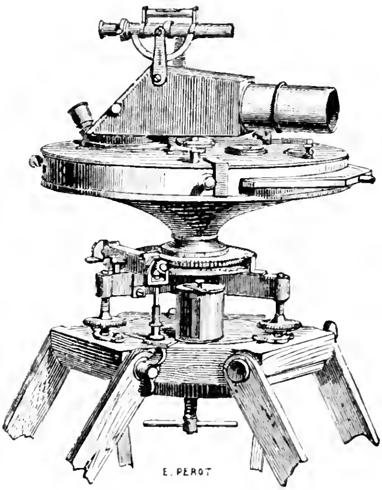
Explosion of Meteorites.—From *Les Mondes* of Feb'y 25th we translate the following:—

M. Swaim proposed before the Academy of Sciences the following explanation of the detonating explosion of meteorites. It has been proved that usually the areolites which reach the sphere of the earth's attraction, absorb a large proportion of hydrogen. Is it not then probable that in their passage through the atmosphere, they also absorb a large amount of oxygen or air, so that the mixtures of the two gases become explosive, and in fact explodes when the temperature either from resistance or the friction of the air becomes of sufficient intensity? We take this occasion to recall that Mr. Swaim, an eminently ingenious and wise observer, mentioned very many years since, that electricity is produced by the rubbing of India rubber or leather belts on pulleys or drums. Also, in 1853, in the third volume of our *Cosmos*, we mentioned a curious experiment which was made by him in New York. Standing on an isolated stool, or on a sufficient number of bottles of glass or earthenware, and holding in his hand a bar or a bundle of iron rods, he approached the conductor to the belt, and then stretching out his other hand, he touched the gas burner, which was immediately lighted. But the greatest discovery that Mr. Swaim has made (who invented and proposed the first telegraphic alphabet, which is now known under the name of Morse), is his system of signalling from great distances, which he has reduced to great sim-

plicity, and which should have been adopted long since by the navy and army administrations.

Photographic Plane-Table.—We have received through the kindness of Mr. James Swaim, a pamphlet on the above instrument, invented by M. Auguste Chevalier, containing full descriptions of its working and construction, and illustrated by engravings and a photograph.

The accompanying woodcut represents an exterior view of the apparatus as given by the photograph. It consists of a photographic objective, behind which is placed a square prism, by total reflection from whose oblique rear surface, the image formed by the lens is projected upon an horizontal sensitive plate set beneath. The lens and prism, as well as an opaque screen covering the plate with the exception of a narrow slit in a plane passing through the axis of the lens and the centre of motion, rotate about a vertical axis in the middle of the instrument, being driven by appropriate clock-work. By this means, when the instrument is placed at one end of a base line, adjusted, furnished with a sensitive plate, and allowed to make a total revolution, it will produce an automatic photographic horizontal projection of all objects within the panoramic field, from which their various angular relations may be determined with the greatest accuracy. It might seem that sharpness of definition was incompatible with a continuous motion of the lens, but the admirable panoramic pictures produced by the apparatus of Martens and of Garella, which operate on the same principle, are a complete and satisfactory answer to this objection.

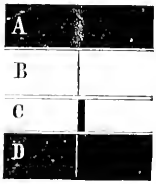


Proper motion of the Stars measured with the Spectroscope.—As we promised in our last number, we will now give the details of this very curious investigation, the results of which have just been published by Mr. Huggins. On page 155 of our last volume will be found a brief statement of the general principle and results involved in this investigation, but we will recapitulate in a few words to make this notice complete in itself. The amount to which a ray is refracted or bent by a prism, varies with the rapidity of recurrence in the waves or vibrations of which that ray is made up. Thus in an ordinary red ray 500,000,000,000,000 waves arrive

at the prism's surface one after the other, in a second; in a violet ray 733,000,000,000,000 in the same time. On this account, the violet ray is more bent than the red. But suppose that the prism were running towards the red ray at such a rate as to overrun 233,000,000,000,000 waves in a second. (This would demand a velocity of 90,000 miles per second.) Then it is clear that it would encounter 732, &c. waves in a second, 500, &c. being due to the motion of the ray towards it, and 233, &c. to its motion toward the ray. Thus this ray would no longer be refracted as red light, but would go to the place belonging to violet light, and in fact as far as any body *moving with the prism* 90,000 miles per second was concerned, *would be* violet light. This is of course assuming an extreme case. Such a velocity (about 360,000 times as great as that of a cannon ball), is out of the question; but with less velocities, a similar effect (though vastly less in degree), would be produced. Thus, if we were *approaching* a star, its various rays should be displaced towards the violet; if we were receding from it, towards the red end of the spectrum. The entire spectrum moving together, and *extra-visible* rays coming in from either end to take the place of the outside displaced ones, no recognition of such an action would be possible by observation on the color of the spectrum alone. Fortunately, the dark Fraunhofer lines give us a scale for measurement.

If we find a characteristic group of lines in the light of a star, similar to a like one in the sun, we may justly regard them as identical in original character, and if these are very slightly displaced, towards one end or other of the spectrum, we may refer this displacement to such a motion as has been described.

Now, the motions observed among the stars in directions at right angles to our line of sight, vary from 30 to 60 miles in a second. These, if in the line of view (*i. e.* toward or from us), would cause very slight displacements in the position of rays in the spectrum, much too slight to be observed with an ordinary spectroscope, but by employing one of great power, made up of two direct-vision spectroscopes and three prisms of 60°, such displacements might be measured. With such an instrument, Mr. Huggins found that the line in Sirius corresponding with the solar line, F, was displaced towards the red by an amount which, making allowance for the earth's orbital motion, &c., would represent a recession at the rate of twenty-six miles per second. In the Figure C is the line in the spectrum of Sirius, B, that in the sun.



Solar Prominences seen without an Eclipse.—The following notice, by Mr. William Huggins, F.R.S., is found in the *Proceedings of the Royal Society*, No. 109, which we have just received.

Last Saturday, February 13th, I succeeded in seeing a solar prominence so as to distinguish its form. A spectroscope was used; a narrow slit was inserted after the train of prisms before the object-

glass of the little telescope. This slit limited the light entering the telescope to that of the refrangibility of the part of the spectrum immediately about the bright line coincident with C.

The slit of the spectroscope was then widened sufficiently to admit the form of the prominence to be seen. The spectrum then became so impure that the prominence could not be distinguished.

A great part of the light of the refrangibilities removed far from that of C was then absorbed by a piece of deep ruby glass. The prominence was then distinctly perceived,—something of this form.

A more detailed account is not now given, as I think I shall be able to modify the method so as to make the outlines of these objects more easily visible.

Editorial Correspondence.

TIN IN MISSOURI.

Prof. Henry Morton.

DEAR SIR:—From an examination of the supposed tin region, from which I have this day returned, I am surprised that the district has not received the attention it deserves from the mineralogists and metallurgists of this country. I have found that the various trappean injections associated with the pre-selenian rocks of South-East Missouri (and cotemporaneous with the formation of the Pilot Knob and Iron Mountain ore deposits,) are more or less charged with stannic acid, and that the granites which they penetrate, have also been influenced to such an extent that they show small amounts of tin.

Several assays conducted on the ground with extraordinary care, and of samples drawn by myself from the deposits, have shown an amount of tin ranging between 0·4 per cent. and 1·78 per cent.

In the next number of the *Journal*, I propose, with your permission, to give a fuller account of the mode of occurrence of the deposits, as well as of their lithological characters, accompanied by full analyses of samples brought by myself from the locality.

Very respectfully yours, CHAS. P. WILLIAMS.

Philadelphia, June 2d, 1869.

Civil and Mechanical Engineering.

BELTING FACTS AND FIGURES NO. II.

By J. H. Cooper.

(Continued from page 329.)

Driving power of Belts compared with that of Friction Gear.

Newton's Journal for 1857, Vol. VI, New Series, p. 163, presents Mr. James Robertson's paper on *grooved surface frictional gearing*, from which we take the following:

"The object of this paper is to describe a system of frictional gearing recently introduced by the writer, intended chiefly for high speeds; and to give such information regarding its action and driving capabilities as the several applications of it in use will afford.

"The grooved surface frictional gearing consists of wheels or pulleys geared together by frictional contact, communicating motion independently of teeth or cogs; the driving surfaces are grooved or serrated annularly, the ridges of one surface entering the grooves of the other. The extent of contact is thus increased in the direction of the breadth of the rim, and a lateral wedging action is obtained, which augments the effect of the pressure holding the wheels in gear; the necessary amount of which is felt to be so injurious to the bearings of the shafts when the power is communicated by plain driving surfaces.

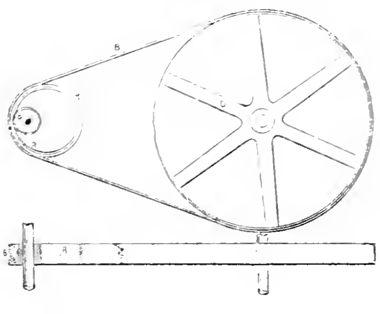
"The grooves are made V-shaped, and are found to suit best when formed at an angle of about 50°. The pitch of the grooves is varied to the velocities of the wheels and the power to be transmitted; the smallest pitch employed is $\frac{1}{8}$-inch, and that required for the heaviest operations about $\frac{3}{4}$-inch. The ordinary pitch is about $\frac{3}{8}$-inch. The wheels are turned up truly, and the grooves equally pitched and made exactly alike on each face; so that on applying the surfaces to each other a well fitted contact throughout the faces is obtained. In order to increase and sustain the wedging action, the points of the ridges are left blunt, to prevent them from reaching the bottom of the grooves.

"Cast iron has as yet been the only material used in the construction of grooved wheels, and its action has been found so satisfactory that there is no necessity for trying any other. The sur-

faces, after working a short time together, assume a smooth polished appearance, taking a greater hold in proportion to the smoothness they acquire; and when a sufficient breadth for the speed and power to be transmitted gets into contact, there is afterwards no perceptible tendency to wear.

* * * "The points that have to be attended to, so far as the power or driving contact is concerned, are the angle of the grooves and the pressure holding them in contact: the extent of surface in contact being determined so as to prevent abrasion and withstand the wearing action. * * * * * Wheels of large diameter show a decided superiority of action.

"In order to obtain a high speed from a driving belt, without the usual arrangement of counter-shafts and belt-pulleys between the main driving-shaft and the machine to be driven, and without the disadvantage of passing the belt over a small pulley, a small grooved pulley is keyed on the shaft to which the high velocity is to be communicated, and upon it is placed a loose, inflexible ring, of two or three times the diameter of the pulley, grooved internally to fit it, and turned up smoothly on the outside to receive the driving belt. The belt gives motion to the speed-ring, the inner grooved surface of which communicates a higher speed to the pulley. The speed-ring is held in effective driving contact simply by the tension of the belt. For obtaining increased lateral steadiness at high speeds, a double speed-ring may be used, if required. By these arrangements a belt may be passed over a speed-ring of 16 inches diameter, and yet communicate the same speed to the shaft as if it were passed over a pulley of only 4 inches diameter.



In the "cut" the driving pulley, D, carries a belt, B, which passes over and drives the speed-ring, R, of large diameter, as compared with the small grooved wheel, P, increasing circumferential contact thereby.

Enlarging the diameter of the speed-ring will permit the driving pulley to be placed nearer to, without altering the speed of the driven wheel, P, and will also increase the adhesion and driving power of the belt.

"Clutches may also be arranged, for engaging and disengaging by means of grooved surfaces.

"In applying this system of driving, where a reverse motion is required, a disc, having an outer and an inner rim, is keyed to the main driving shaft—the outer rim grooved on the inside, and the inner rim on the outside, with corresponding grooves. The shaft that is to be driven carries a small grooved pulley, the diameter of which is slightly less than the distance between the two grooved rims. The motion of this pulley will be reversed by moving it slightly nearer to or farther from the main driving-shaft, so as to throw it into gear with the inner or outer rim respectively. * * * * * *

"For comparing the pressure required to hold the grooved surfaces in gear and the power transmitted, various opportunities have occurred in the actual use of the frictional gearing, and arrangements have been made for purposes of experiment. One method of comparing its driving capabilities with those of belts is directly obtained by the simple speed-ring movement already described for raising high speeds. One of these speed-rings has been working satisfactorily on a large foundry fan for some time; and from the circumstance that the fan was previously driven by a belt of the same size, over a plain pulley of the same diameter as the small grooved pulley now used, this case affords a certain practical means of comparing the efficiency of these two methods of communicating motion. Before the application of the ring the belt was passed over a pulley 6 feet diameter, keyed on the driving shaft; and over a pulley 7½ inches diameter, on the fan spindle; but the continual bending of a large heavy belt over a pulley of so small diameter made it difficult to keep up the proper driving tension, and the belt was speedily cut up. The ring now interposed between the belt and pulley is 13½ inches diameter, and saves the belt from injury by the greater diameter over which it bends. The ring works steadily, and drives the fan at the same speed as when the belt was passed directly over the small pulley; thereby showing that the grooved metal surface does not strain the bearings more than the ordinary arrangement of driving by belts.

"Another method has also been employed for comparing the driving capabilities of the grooved-surface gearing with those of belts, by means of a testing apparatus, having the same pressure on the bearings of the axis as is produced by belts. The testing appa-

ratus is made by gearing together two spur grooved wheels, each 21 inches diameter and $3\frac{1}{2}$ inches face: the grooves being cut $\frac{3}{8}$-inch pitch, and at an angle of 50°. Motion was communicated to the driving-wheel by a 7-inch belt, over a pulley 30 inches diameter, so disposed that there was no pressure to hold the two wheels in gear but the pull or strain of the belt. A plain friction strap wheel was keyed on the spindle of the driven wheel, by a strap and break handle attached, so that it could be either retarded or stopped. On applying the break, it either caused the belt to slip or the driving engine to stop, without the grooved wheels showing any tendency to slip.

"There is a slight slip in the rolling action of the grooved wheels which does not occur in the action of plain surfaces, which arises from the difference of the diameters of the points of the ridges and bottoms of the grooves; but this slipping is little felt in practice, and, when measured, is inconsiderable in amount. In a pair of grooved wheels, 8 feet diameter and 1 foot broad, with 24 grooves working together, there is a slip of only 10 square inches in an entire revolution; whereas, in toothed wheels of the same breadth and diameter, with cogs of 3-inch pitch, of the ordinary proportions, there is a surface to slip over on each cog of about 24 square inches, or nearly the entire area of one side of the cog; making a total slip of about 16 square feet in every revolution.

"Lengthened experience is necessary to ascertain the smallest breadth of face that will be sufficient for transmitting a given amount of power without abrasion or wearing action; and it is therefore preferred at present to make the grooved wheels broader in every position than seems to be absolutely necessary. The general proportion of toothed wheels, as regards both breadth of face and other dimensions, are sufficiently strong for transmitting the same power of grooved surfaces; but the writer is of opinion that less breadth of face and lighter proportions of arms and rims can be used with safety. If the grooved wheels are employed in every position in a factory where wheel gearing is required, no shocks or jolting action can take place; and therefore all the wheels themselves, and also the shaftings and supports, may be made much lighter than can be used with ordinary gearing.

"One of the principal advantages of these grooved wheels is their smoothness of action, in positions and at speeds when ordinary toothed gearing produces a disagreeable jarring noise, their action is scarcely audible."

Mr. James Christie, of Pittsburgh, Pa., has communicated the following: "I can give an example of engine and connected belting, during the performance of which I had frequent opportunities of observation.

"The engine has a cylinder, 8-inch diameter by 12-inch stroke, and made 150 revolutions per minute under a boiler pressure of 75 pounds to the square inch, and piston pressure of 40 pounds, doing an effective service of 18 horse power. It drove a brick machine by a first-class double leather belt, 6-inch wide and nearly $\frac{3}{8}$-inch thick, laced in the usual way. The smooth turned iron driving pulley or engine shaft is 22-inch diameter; the driven pulley, of like material and finish, on brick machine, 48-inch diameter; distance, horizontally, between shafts, 12 feet; top fold of the belt slack, no tightener applied, belt was well stretched, no resin or unguent used.

"Both engine and belt were insufficient for the purpose, but I considered each about the equal of the other. When first used the engine would drive ahead and slip the belt; as belt and pulley became more *attached*, and belt was tightly laced, the belt would 'stall' the engine."

The above gives 24·27 square feet of belt per horse-power per minute, and 114·8 pounds strain to one inch width of belt, and may be considered as a very reliable example of a hard worked belt.

"In driving 8-inch trains of rolls, the practice here is, when driving direct, to use engines of from 12 to 16-inch diameter of cylinder, similar in construction to the usual slide valve engines the country over, and working under a boiler pressure of say 90 pounds to the square inch, and average piston pressure of 50 pounds. The 12-inch cylinders have proved inadequate, the 14-inch have ample power, and the 16 inch have an overplus.

"The piston speed of these engines is invariably high, seldom less than 400 feet per minute, frequently 600 and 700, and sometimes as high as 800.

"Fourteen inch belts have proved insufficient in driving such a train, 16-inch do very well, and 18-inch are wider than needed. The 16 and 18-inch are, however, commonly used, and are always double leather or two or three-ply gum belts.

"When driven from line shaft, which is the usual plan, 6-foot pulleys are used, both on line and on rolls, say 25 feet between centres, and the angle of belts with a vertical line about 20°. Some-

times the pulley on rolls is made heavy, but commonly separate fly-wheels, of about 8 feet diameter and 4 tons weight, are used. Tighteners are seldom employed, speed of rolls from 150 to 250 revolutions per minute.

"In a steel mill here, a 20-foot fly-wheel pulley on engine shaft, running at 60 revolutions per minute, drives a 6 feet pulley, 4 tons weight, on the train at 20 feet distant, between shafts, horizontally. The train is a 9-inch one, belt 17 inches wide, of three-ply gum, and no tightener used.

"The belt slips while rolling long lengths, which would indicate insufficient momentum in and lack of belt contact on the fly-wheel pulley on the train.

"Greater distance apart of shafts and less difference of diameters of pulleys would give, therefore, a better belt result.

"I have never heard of any objection to the use of belts in rolling mills on the score of durability.

"In order to obtain the full value of a belt, it is first necessary that it should be in thorough contact with the pulleys. A new belt will be found touching in spots, and will not pull well for want of entire contact with pulley; any unguent put on the belt will be found of immediate benefit, as it softens its surface, and brings it in complete contact with the face of pulley. The hair side of belt, on account of its smoothness and closeness of texture, seems to conform to this necessary condition much sooner than the flesh side, which is open in texture and rough on the surface. But after the belt is once worn to the proper condition, I doubt if there is any appreciable difference in the two sides, in value. In fact, with *well worn* belts, which have been used alternately with each side to pulley, it is often difficult to distinguish the hair side from the flesh side. By *well worn* I do not mean *injured by use*, but simply that condition of belt in which the color of the sides is rendered uniform by absorption of oil, and in which the surface gloss and texture is made nearly uniform by contact with pulleys. Intimate contact between belts and pulleys is undoubtedly necessary. The utility of smooth faces to pulleys is also well established."

(To be continued.)

CORNISH ENGINES.

By W. H. G. West, Engineer, U. S. N., (N. Pacific Squadron.)

If another paper upon this subject, coming from so far over the sea, can be regarded as worthy of a place in the *Journal*, I shall be glad to explain some expressions in my paper, published in the October number of 1868, that Mr. Henderson appears to misunderstand, and to further support my reasons for the superior duty performed by the Cornish Pumping Engine.

It was not my intention to find fault with anything Mr. Henderson had written, but to add other important reasons for the superiority of the engine, which I had found true in my practice, as well as in that of many other engineers, and thus to aid him in making its proper construction more generally understood.

Upon reading Mr. Henderson's paper carefully, before preparing my first article upon the subject, I perceived what appeared to be inconsistent expressions, and a weight given to apparent influences not warranted, in my estimation, by either philosophy or general practice.

Trusting that he will pardon me for allowing myself to be drawn into even the semblance of an unfavorable criticism, I must beg him to generously and charitably construe any of the following remarks that may so impress him, assuring him, at the same time, that my desire, as well as his, is to have the Cornish engine appreciated, but, in addition, to have the rotative engine understood.

On page 371 Mr. Henderson says, though not in these words, that the shortest way is the best way. Why, then, is not the "Bull" engine, which is straight as the diameter of a circle, better than the beam engine, forming, as the latter does, three sides of a rectangle. But this is no better than running a ship against an iceberg, instead of going around it, because going through is the shortest way.

In his first paper, page 34, he says that the beam engine does better duty than the Bull engine, because it has a greater weight of material in the moving parts, and that *the duty varies with the weight.* Now, this is a remarkable and valuable discovery, as we can at once save half the coal by doubling the weight, and all the coal by making the engine infinitely heavy.

On page 371 Mr. Henderson says: "Turning my attention to minor points in the case, as prominently brought forward by Mr. W., I find that having at one place recommended a *uniform speed*

of piston for pumping water, I have afterwards acknowledged that the speed of the piston, when it is *not* pumping water, is uniformly accelerated, *i. e.*, making the in-door stroke." What, then, means the following quotation from his first paper, page 31, last paragraph? "In the out stroke the weight of the plunger descends with a constant resistance, and consequently a *uniformly augmented velocity.*" The latter statement is the true one; and, as steam also accelerates the motion of the piston, the stroke of the rotative and the pumping stroke of the Cornish engine are virtually the same, while the steam strokes are exactly the same where we have a very light fly-wheel. And I must beg Mr. Henderson to remember that a peculiarly Cornish apparatus, called the lifting pump, is much used in mines; that it is so arranged as to lift water while the plunger is rising, or during the steam stroke; and that it is much more easily deranged by rapid motion and jarring than is the force pump.

On page 32 we find that a load may not be "capable of acquiring momentum, as is the case, to a considerable extent, in pumping water." Is there no *vis viva* in the stream of water issuing from the hose-pipe of a steam fire-engine? Is there no potential energy in the single heavy sea that, thundering across a strong ship's deck, sweeps everything clean, fore and aft? Momentum means nothing when we speak of the power to do, or of the work done, and is entirely misapplied in this discussion. A column of water moving through a pipe, after having received an impetus from the plunger, will retain its motion until stopped by friction, turns, etc. This occurs during the latter part of the stroke of either engine, while the speed of piston rapidly falls off, and, creating a vacuum, the plunger will be forced into the space so formed by atmospheric pressure. The effect is to, in part, equalize the speed, but it probably will not favorably affect the economy, for, as in all other cases of such interchange of energy, the water must lose some of its stored up work in overcoming friction and other obstructions.

I am gratified to see that on page 372, first paragraph, Mr. Henderson adopts my opinion, as expressed on page 258, last paragraph, that the terminal pressure need not be equal to the load, and so disowns his former statement, page 34, first paragraph, that "the potential energy is gradually converted into actual work, until the end of the stroke, when it should be entirely exhausted, the *terminal pressure* will then not equal the *load and back pressure.*"

As Mr. Henderson is desirous of submitting the question to the readers of the *Journal*, he will probably not object to an investigation of the last full paragraph on page 372. In it we find that because the direction of a motion of a weight is horizontal, there can be no *vis viva*, and all this battering with shot and ramming of ships, during the war, is a dream. The cruelly satirical acuteness developed in the construction of that paragraph, did not show the author of it, that had the moving parts of that pump only weighed one pound, they must have had what he understands to be *momentum*.

On page 372, third paragraph, I find: "It cannot descend in the manner described, as at that time it is going up." That depends upon whether he was comparing the Cornish engine with the rotative, or the beam engine with some other not mentioned. The pistons of some Cornish engines do undoubtedly descend with the plungers.

In regard to cushion, spoken of in the same paragraph, I have cards taken from engines which went over the centres, without showing any difference of speed, against 20 pounds of steam, above zero, when the initial was only about 17·5 pounds above atmospheric pressure, and that without a fly-wheel. Other cards taken from the same engines show an initial pressure of 13 pounds above zero, and a cushion pressure slightly above. Reference to almost any work on indicators will show cards on which the cushion pressure is above the initial. These cards are, of course, all taken from rotative engines. The Cornish engine never has so much cushion; it is not necessary, and the little steam that is compressed loses, through condensation, much of the *vis viva* imparted to it, while waiting for the steam stroke. The Cornish engine rests some time in this position, but the piston of the rotative engine rebounds from the compressed steam like a billiard ball from its cushion, and is kept going by steam from the boiler, admitted at the commencement of the stroke by the *lead* of the valve. If Mr. Henderson will make a few more experiments in the valve setting he recommends to us, he will find that a valve may be so set as to give lead and cushion, even up to the boiler pressure, and the engine may still "be persuaded to pass its centres."

On page 373 I find that the fly-wheel, swinging around with its comparatively great velocity, has very little *vis viva*, but that the "bulk of it" passes to the immovable foundation. If the outer

bearing of the shaft be braced from the bed-plate, and the pump rod be connected directly to the piston rod, no *vis viva* can pass through the crank pin to the foundation. Where the pump is made fast to the bed-plate, no holding down bolts are needed; the bed-plate will not move upon the foundation, and therefore no work is passed from one to the other. No *vis viva*, then, can pass from the crank pin to the foundation, unless the pump is driven from the shaft. The bed-plate receives some of it through the other parts, but returns it by the same path; for, receiving the "bulk of it," according to Mr. Henderson, the form of the bed-plate must undergo a change by crushing, or extension, and will shortly become disintegrated, and fall to pieces, unless it is elastic and can regain its original form, when it will return the *vis viva*. This I did not question, but am constrained to repeat that not the smallest imaginable quantity goes to the fly-wheel, or anything else, at the "dead point." Between the ends of the stroke it is given, through the crank pin, to the wheel and other parts, and is much more faithfully returned than it can possibly be by *continually condensing steam.*

On page 374, Mr. Henderson tells us that steam of from sixty to seventy pounds pressure is used in Cornish engines, and, on page 34, that at the end of the stroke the potential energy of the steam should be entirely exhausted; this cannot be until we have a pressure equal to zero. What is the grade of expansion, and the pressure of the cushioned steam? In the Cornish engines which it has been my fortune to see, the initial pressure has not been so great, nor has the expansion been carried so far. By visiting Messrs. Merrick & Son's machine works, he will find a very large rotative pumping engine in course of construction, designed to work under a pressure of 65 pounds above zero, with eight as a factor of safety of material. It will, therefore, be perfectly safe to apply a much higher pressure, should the water increase more than is anticipated by the designer. Other condensing engines, using high pressure steam, may be found in Pennsylvania.

From the same paragraph we may conclude that Mr. Henderson's experience has been amongst rolling mill fly-wheels, and that he finds difficulty in appreciating the light one so often recommended. The immense strain that any variation of speed in such a heavy wheel would bring upon the engine, can only be compared to that which is brought upon the Cornish engine by the inertia of the

massive beam, with its accompanying connections, of extra strength and cost.

Following this, we have a proposition to decide, which of two engines is the most economical; the engines to have the same cylinder capacity, or, which is the same thing, one having half the power of the other, to do the same work, with equal quantities of fuel.

We next have, by the use of a "rough model," very thoroughly proved to us, that, without a fly-wheel, the engine passes a considerable part of its *vis viva* to the hand representing the foundation, but that by substituting for the crank a fly-wheel with two arms, the loss is so small as not to be noticeable. This Mr. Henderson has proved by "actual experiment," but was not the experiment a little too rough for such a clear and impressive conclusion, and is not that conclusion, page 374, exactly—wrong?

Regretting very much the necessity of drawing attention to the complimentary remarks on page 375, first paragraph, I must beg the readers of the *Journal* to notice Mr. Henderson's singular forgetfulness of the various lifting pumps scattered through such an immense mine as Fowey Consols; many of them pumping water during the steam stroke, as hereinbefore mentioned.

The proof contained in the last paragraph of page 377, that, "2 and 2 being four," the rotative engine loses more by condensation in the cylinder than the Cornish engine, is not quite clear to my mind; but, if Mr. H. will make the computation, he will find that because, in two engines of the same power, the Cornish cylinder surface is much larger than that of the rotative engine, and because the mean differences of temperatures, for the same times, are nearly the same, the Cornish cylinder, *as a condenser*, stands to that of the rotative as about 1·38 to 1.

To the remarks on page 376, in regard to the fullest economical expansion, a question will probably be the best answer. Can expansion be carried to the same extent, economically, in two cylinders, one immersed in ice water, and the other surrounded by hot gas? Engines built from the same drawings and patterns will never do the same work, with equal amounts of fuel. There are grades of expansion suited to each engine, for different states of steam pressure, packing, journals, and other influences. The *highest* grade of expansion is not always the most *economical*, but, were it so, perhaps it would be just possible, in designing, to

lengthen the stroke, when we apply high pressure steam to the rotative engine.

The supposed discrepancies which Mr. Henderson has discovered, and quoted on page 376, paragraphs three and four, I fail to see. The jacket being open to the boiler pressure must have the same temperature of steam, but that this cannot prevent steam from condensing in the pipe and jacket, may be proved by the common steam heater.

In responding to the invitation contained in Mr. Henderson's last paragraph, page 376, I shall have to offer pounds of coal per horse power per hour, but from that he can estimate the duty, approximately. In Rankin's Treatise on the Steam Engine, page 409, he will find that with Rowan's engine, burning ordinary coal, the consumption was only 2·07 pounds per horse power per hour, and with an evaporation less than the best obtained from the boiler, only 1·12 pounds was consumed per horse power per hour, giving a duty away beyond all Cornish engines, though it is well known, amongst steam engineers, that the Cornish pumping engine is, in general, the most economical.

As soon as they can be procured, I shall have pleasure in sending to the *Journal* the performance of some American engines. They will not, perhaps, equal that which I have presented, but certainly will compare favorably with the majority of ordinary Cornish engines.

It must not be forgotten that the exceptional performance of the 80 inch, at Fowey Consols, lasted but a short time,.and has never been equaled by any other of her type.

In conclusion, I respectfully reiterate my assertion—"the man who can make a good rotative engine *uderstandingly*, will not fail to make a good Cornish engine."

Steel rails, it is reported, are to be laid on the entire length of the railroad from Paris to Marseilles. The change from iron to steel will require 137,000 tons of steel. From experiments made by the company, it has been calculated that in the vicinity of the stations iron rails will not last over four years, and on the whole line not over eight or ten years. The steel rails it is believed, will last thirty or forty years. The bridges are also to be constructed of steel as soon as iron ores suited to the manufacture can be obtained in sufficient quantity.

THE NATURAL CONDITION OF OUR GULF COAST HARBORS, CONSIDERED WITH A VIEW OF THEIR PERMANENT IMPROVEMENT.

By D. S. Howard, C. E.

One universal condition of our gulf coast harbors is their sandy bottoms and surroundings.

All the rivers emptying into the gulf for many centuries past, have been bringing down large quantities of sediment consisting of sand, heavy enough to sink in salt water, and light enough to form a very gradual slope to the bottom, together with a lighter silt driven on shore, forming the rich bottom lands now extending many miles inland, more or less, throughout the whole coast. This shoal formation extended into the gulf, until the current of the ebb tide from the shoal bays, estuaries, streams, etc., meeting the flood tide, where the breakers are first formed, threw down a deposit of fine sand, forming a reef, which accumulated until the shoals are separated from the deep water by dry land, except a pass, which is narrowed and shoaled by the action of the sea on the sediment, until its smallest section is barely sufficient to admit the in and out flow, caused by the winds, tides, rain-fall, evaporation, etc.

These bays have become very general along the whole gulf coast, forming harbors more or less valuable, according to the more or less water passing in and out from the above named causes.

It will be readily observed that the larger the surface of the bay, provided it be deep enough to preserve its level, while the flood tide enters, the more tide water it will contain at high water, consequently the more salt water must pass out by the ebb tide, in the same length of time. Salt water being heavier than fresh, will be more effective in scouring the sand in the passage, the section of which will bear a certain proportion to the amount of water passing through it in a given time.

This law is well illustrated by the known conditions of all the harbors on the gulf coast and other similar locations.

Among the most conspicuous examples, is the Pensacola harbor, with but one outlet for a surface of about three hundred square miles, with three considerable rivers entering the bay, which has abundant depth of water throughout. The free circulation of salt water through the Santa Rosa Sound, also has a good effect on the open-

ing of the harbor, like that of Long Island Sound on Sandy Hook, it being similarly situated.

The improvement now going on at Hurl Gate, will undoubtedly improve Sandy Hook in the same proportion, by drawing a larger amount of tide water from the sound through Sandy Hook. Whoever has passed through Hurl Gate when the tide is going out, must have observed the rocky obstructions nearly amounting to cascades. By enlarging the section of the sound at Hurl Gate, more of the tide water will pass out at Sandy Hook in ebb tide, and more in, at flood tide, which will scour the channel, by the increased motion of the water, until the increased section of the channel shall reduce the velocity of the current across the bar to its former force.

Mobile Bay, Sabine Lake, Galveston Bay, Metagorda Bay, Aransas Bay, and Laguna Madra, are all examples illustrating the law that controls the outlets of such harbors, but of the opposite kind to that of Pensacola Bay, being all large sheets of water, but so shoal in many places that there can be little or no tide reaching their inland extremities, the water being so shoal, that before the flood tide reaches the inland shores of the bay, the ebb tide commences to pass out with less than half the force than would have been obtained if the flood tide had have been free to preserve a level surface throughout the bay,

Therefore whatever will open up these bays to a free circulation of the tide from one deep place to another, will materially increase the depth of the entrance to the harbor.

The contemplated ship channel from Galveston to Houston, fifteen feet deep, and one hundred and fifty feet wide, is well calculated to improve the entrance to Galveston Harbor, by opening up the deep places in Galveston Bay, to Buffalo Bayou, a deep sheet of water extending about twenty miles farther inland.

The large surface which this improvement will open up for the flood tide to fill, will draw in more than double the amount of salt water now entering the bay, and afford the facility for it all to pass out in the same length of time, except the obstruction of the bar at the outlet, which will create double the velocity of the current over the bar, until it shall have been scoured to a section proportional in area to the additional amount of water passing in and out over the bar, when the original lesser velocity will be restored.

When we consider that the weight of the sand on the bar was barely sufficient to overcome the velocity of the water which left it

there, we arrive readily at the conclusion that a slightly increased velocity of water passing over it will disturb its state of rest, and convey it as much further as the increased velocity shall be continued.

Pass Cavallo, the entrance to Metagorda Bay, will be improved in proportion to the facility which the Lavaca Canal, now being opened affords for the filling of the upper bay by the flood tide, which may be so small that there may be no marked difference until it shall have been opened all the way from the pass to a greater depth than the water on the bar, and a width that will give additional freedom to the tide in reaching the extremities of the bay.

A capacious channel into and through Espiritu Santo Bay to the deep part of Aransas Bay, would also add very much to the improvement of Pass Cavallo and Aransas Pass. This route is now navigated with about three feet water.

Aransas Pass would be still more improved by a more free communication between Aransas Bay and the beautiful deep Bay of Corpus Christi, where a channel eight feet deep and sixty-four feet wide has been made, and is now being enlarged to one hundred feet in width. Double that depth, and double the width, would make Aransas Bay one of the best harbors on the gulf coast.

A continuation of these improvements through the shoal waters of the Laguna Madra to Point Isabella, would be all that is required for the gulf coast harbors, while the inland navigation afforded by the improvements would more than pay all expenses.

In addition to the double pecuniary advantage of such improvements, we must add the effect on the climate of the country produced, by admitting tide water to the inner coast, and some distance up the sluggish streams, emptying into the gulf.

A country with such great natural advantages as that which relies upon the gulf coast for an outlet for its products, can never be fully developed, without better harbor facilities than are now found on this coast. No vessel drawing water enough to be a safe navigator of the gulf, can enter any of the harbors on the coast, except Pensacola. The condition of this harbor is well calculated to establish the position here taken, with regard to the improvements of other harbors, with similar surroundings.

The soundest political economy would seem to dictate the proper course for our general government in this matter. Not only are the whole United States interested in the prosperity of the country dependent upon the gulf coast, but all the nations of the earth who

have any intercourse with us. The whole of our profitable exports have ever been and ever will be the products of that country. Let these improvements be made, and a stimulus would be extended to all of the other states, that would procure a return of more than our former prosperity.

Lyons Falls, N. Y., April 1869.

EXTRACTS FROM AN ENGINEER'S NOTE BOOK.

By W. M. Henderson, H. E.

(Concluded from page 319.)

On the construction of Steam Boilers, and rules relating thereto.

Care should be taken to facilitate the uninterrupted ascent of the steam from the point of formation to the surface, with a proper circulation of the water to take its place. Vertical heating surfaces, as the inside box of the locomotive type of boiler, should be so angled, that the globules of steam formed thereon, may leave such surface on ascending, otherwise the multitude of these globules will form a stratum of steam between the water and the plates, exposing the latter to the danger of burning. The water spaces should be large enough to allow the steam generated to rise to the surface, with space for a down current of water outside, to take the place of that just disposed of. In contracted water spaces, the steam in formation, drives the water out, preventing circulation, causing, forming, and endangering these parts to overheating, and burning out, explosion in such case is only a question as to how long a time it will take to destroy the molecular construction of the plates, by alternately overheating and deluging them with water.

This phenomenon may be seen to perfection in the steaming of many of our steam fire engines, the water will surge and re-surge in the glass gauge, as it is displaced from, and returns to the water legs, and it is not an unusual occurrence, after the fire has been withdrawn, for them to continue generating steam in great profusion. The palpable reason for this is, that the water then is allowed to descend into the legs of the boiler, and comes in contact with the super heated plates. An interesting experiment would be, to subject all the boilers of this construction to a test, by inserting a small cock in the outside box, to discover whether, or not, they are subject to this dangerous practice.

The water spaces, for practical purposes, should be from $2\frac{1}{2}$ to 4 inches, according to the size and description of boiler, and thoroughly secured by iron stays, screwed and riveted. In cases where boilers have internal flues, the outer shell will be relieved from a longitudinal strain, equal to their area. Ratio of diameter to shell when one is used, as 1 to 2·5, in no case more than as 1 to 2; when two are used, as 1 to 3. When tubes are employed, the distance between them will vary with the calibre, and should be about one-half of their diameter. Where wrought iron flat heads are used, they should be composed of plates at least one-half thicker than those forming the circumference, and be well secured with corner gusset plates of 45°, and angle irons radiating to the inside circumference of the shell.

As a rule, no hole should be cut in a steam boiler larger than is absolutely required. Where a steam drum is riveted on, it may not be necessary to cut the plate at all; a number of small holes of an aggregate area, equal to the steam-pipe, or safety-valve, is all that is required; and where a man-hole is cut, the margin should be stiffened with strengthening rings, if the frame of the man-hole itself is not sufficiently strong to restore the strength of the plate cut away?

Riveted joints exposed to a tensile strain, are directly, or nearly so, as their respective areas, or in other words the collective areas of the rivets should be equal to the sectional area of the plate, taken through the line of rivets.

The proportional size of rivets, pitch of ditto, and lap of joint, will be as follows: Diameter of rivets for plates up to three-eighths is twice the thickness of plate. The pitch for three-sixteenth and one-quarter inch plates is six times the thickness or plate; for five-sixteenth and three-eighth inch plates it is five times. The lap for three-sixteenth, one-quarter, and five-sixteenth inch plates should be six times the thickness of plate, and for three-eighth plates five and a half times. For double riveting, add two-thirds of the depth of the single lap. Diameter of half round heads one-fifteenth for one-eighth of diameter of rivet. Diameter of conic heads twice the diameter of rivet, and the height of both description of heads, three-fourths diameter of rivet.

As before remarked, the question relating to the strength of cylindrical tubes, when exposed to external pressure, as the flues of steam boilers, require more than ordinary attention, it has been

found by direct experiment, that the strength varies in accordance with a certain power of the thickness, the index of which, taken from the mean of the experiments, is 2·19, or rather higher than the square. The formula for calculating the collapsing pressure, is as follows: where D=diameter in inches, L length in feet, K thickness of metal, and P the collapsing pressure, then $P = \frac{806300 \times K^{2.19}}{L \times D}$ This formula, however, is not of easy solution, by taking 2 instead of 2·19 for the index of K, we get $P = \frac{806300 \times K^2}{L \times D}$ whence the collapsing pressure may be readily calculated by ordinary arithmetic. For thick tubes of considerable diameter and length, it will be found sufficiently exact. For small diameters and short lengths, up to ten feet, the theoretical formula is more correct, but it does not strictly apply to tubes over that length, and when we consider a safe margin is left by allowing only one-sixth for the safe load, the square of the thickness is near enough for all practical purposes.

Rule for calculating the bursting pressure per square inch of a steam boiler in the longitudinal direction. Multiply the area, in square inches, of the cross section of the rim of the boiler, by the value of the iron, as deduced from experiment, and divide by the area of the boiler head.

Formula S = area cross section, C = constant — A = area boiler head, and P = bursting pressure per square inch. Then $P = \frac{S \times C}{A}$

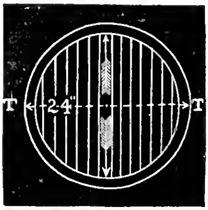
Example.—Given a steam boiler of 34 inches diameter, with single riveted joint, and constructed of quarter inch plates. To find the pressure per square inch, that will tear the shell apart, in the longitudinal direction. Here the sectional area of the rim is equal to 18·66 square inches, the value of the iron for the single riveted joint is 34·000 per square inch, and the area of the boiler head, is 452·39 square inches.

Then $\frac{18{\cdot}66 \times 34{\cdot}000}{452{\cdot}39} = 1502{\cdot}4$ pounds, which is the bursting pressure per square inch.

Rule for calculating the bursting pressure of a steam boiler in the curvilinear direction. Multiply the area in square inches of the section of the plates taken through the axis of the boiler, by the value of the iron, as deduced from experiment, and divide by the diameter of the boiler.

This will be better understood by reference to the annexed diagram representing a section of a steam-boiler. The pressure acting as indicated by the arrows, the resisting material being the sides of the boiler, T T. It is obvious the length is quite immaterial, a hoop of one inch in length will give us all that is necessary to make the calculations, that one inch being equal in effect to any other inch, or aggregate number of inches, when multiplied by any such length taken.

Fig. 1.



Formula.—Let T = thickness of plate, C = constant, D = diameter, and P = bursting pressure. Then P = twice $\frac{T \times C}{D}$

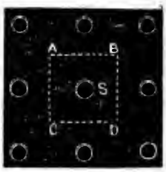
Example.—Taken the same boiler, as given above, to find the bursting pressure per square inch, that will rupture the shell in the curvilinear direction. Here T = 25 inch. The two sides = 5 inch. The value of the iron as before, 34,000 pounds per square inch, and the diameter is 24 inches.

Then $P = \frac{5 \times 34000}{24} = 708{\cdot}33$, which is the bursting pressure per square inch for the safe load, allow only one-sixth of the bursting pressure. It will be seen by comparing the results of these calculations, that the boiler is, as before stated, twice as strong in the longitudinal, as it is in the curvilinear direction.

The manner for proceeding to calculate the strength required in flat stayed surfaces of a steam-boiler, will be readily understood by an inspection of the following diagram, showing the general arrangement of such staying, and the surface of pressure each stay has to sustain.

It will be observed that the distance from the centre of one stay to the next, forming a square, is the only measurement of surface necessary to be calculated, each square of the entire surface being alike, and assuming this rectangle to be situated as shown by the figure, A B C D, we find that the whole of the pressure bearing upon this surface, has to be sustained by the single stay, S. All that remains then to be done, is to so proportion the area of this rectangle in reference to the pressure of steam to be employed, and to make the cross section of the stay, at the smallest part, strong

Fig. 2.



enough to bear six times the pressure that will be brought to bear upon it, taking the ultimate tensile strength of iron stays at 60,000 pounds per square inch, and assuming that such stays are screwed and well riveted on both sides. The additional strength secured by riveting the stays has been found to be about 14 per cent.

Safety valves allow one square inch area of safety valve per each 3 square feet of grate surface. Calculations for the safety valve.

1st. When the lever is parallel, multiply the weight of the lever by half the number of leverages. If the lever is not parallel, balance it upon a fulcrum, and multiply the weight, as before, by the number of leverages contained up to the point of balance, add the weight of the valve and attachments, and divide this product by the area of the valve in inches.

The result will give the weight per square inch upon the valve seat, from the effect of the lever and valve, etc., alone. This is a constant weight entirely distinct from the effect produced by the sliding weight of the ball, which will vary with its position on the lever.

2d. Divide the weight used, by the area of the valve in inches, this will give the direct weight per square inch, supposing it to be placed immediately over the valve; if it is placed upon the second notch, it will be twice as much; if upon the third notch, three times as much, and so on, depending upon the point of location distant from the fulcrum. Wherever it is placed, divide such distance from the fulcrum by the distance from the fulcrum to the centre of the valve; this will give the amount of leverage by which to multiply the *direct* weight previously found. The *constant* weight must always be added at all points where the lever is required to be marked, or the weight to be ascertained.

There is little further to add to this subject of steam boilers, rendered as brief as possible, commensurate with the extent of information attempted to be conveyed, if not therefore generally instructive, it will, at all events, like a short sermon, not prove tiresome to the reader.

One other item, without which the boiler is not complete, is the *chimney*, which we will now briefly dispose of. The draught, or current of air passing through a furnace, is occasioned by the difference in weight between the column of rarified air within the chimney and that of an external column of the same proportion.

The area of the chimney, or smoke stack, as already given, should

be about two square inches per pound of coal consumed per hour, or twenty-four square inches per square foot of grate surface. The length should be about one yard per inch of the diameter. The general practice of decreasing the area towards the top is an error; they should be constructed of at least an equal area throughout; for although it is true the volume of the heated gases diminish as they leave the furnace, the velocity is also proportionally reduced.

Philadelphia, March 1st, 1869.

Industrial Products of Prussia.—The following statistical details are given in the *North German Correspondent* on the industry of Prussia:—"In the year 1867 the number of iron-smelting works was 1,211, employing 87,086 workers, and the entire value of the products was 115,678,648 thlrs. These industrial establishments produced unwrought iron to the value of 19,789,481 thlrs., raw steel-iron 2,022,200 thlrs., cast iron 12,924,914 thlrs., bar and rolled iron 28,267,858 thlrs., sheet iron 5,810,333 thlrs., iron wire 2,588,342 thlrs., and steel 19,351,574 thlrs. The steel industry has made enormous progress in the last few years, and Prussia in this branch is now unsurpassed. The celebrated establishment of Krupp, in Essen, has a world-wide reputation, and seems to have brought the manufacture of steel to the highest attainable perfection. With regard to the remaining metallic products of Prussia, the respective value of the quantities obtained in the same year were the following:—Silver 2,756,455 thlrs., lead 4,739,812 thlrs., copper 3,739,440 thlrs., brass 1,269,339 thlrs., zinc in bars or plates 7,982,491 thlrs., sheet zinc 2,630,317 thlrs."

The Junction of the Pacific Railroads gained additional commemoration by the delay that carried it over from Saturday to Monday. California marked the event at its full value to her, and that is almost beyond estimate. A serenade by thirty locomotives in unison was one of the features at San Francisco that must be more striking to read of than it could have been agreeable to hear. The military and civil pageants in that city, and in other cities of that State were probably equal to any that the State has ever seen. The other cities of the country, north, east and south, responded fittingly, and the occasion was everywhere noticed in a manner attesting the sense of its value.

Mechanics, Physics, and Chemistry.

REVIVIFYING BONE BLACK, BY BEANES' PROCESS.

In a previous issue of this *Journal*, Vol. XLIX., p. 428, we called attention to this process. We now extract from the *Chemical News* a series of letters, which throw important light on the same subject. In the first place, we give a letter from Mr. P. Casamajor, of New York.

To the Editor of the *Chemical News*.

Sir:—In No. 472, of Volume XVIII. (*Am. Repr.*, Feb. 1869, p. 78), of your excellent publication, is an able and exhaustive paper, by Dr. Wallace, "On the Chemistry of Sugar Manufacture and Sugar Refining," wherein occurs a passage to which exception must be taken, as it is of a nature to create false impressions.

After speaking of the injurious action of weak acids which invert cane sugar, and enable liquors to dissolve iron from animal charcoal and from iron tanks and cisterns, besides other impurities, Dr. Wallace adds, "In this way Mr. Beanes' process for treating animal charcoal with hydrochloric acid gas, although otherwise all that can be desired, has entirely failed, and has caused ruinous expense to some refiners who have used it."

While admitting the premises of Dr. Wallace, I fail to see how he arrives at his conclusion. Were it true that Mr. Beanes' process produces weak acids, the conclusion would be perfectly legitimate.

I feel authorized to speak on the subject of Mr. Beanes' process for purifying bone-black. I was one of the first in the United States who became acquainted with the process, and for the last two years I have had charge of it at the Refinery of Messrs. Havemeyers & Elder, in Brooklyn, E. D., opposite New York. They refine 100 tons of sugar daily, and treat weekly 220,000 pounds of bone-black by the process of Mr. Beanes. They have now three large apparatus for making the hydrochloric acid gas in constant operation, and are putting up more retorts.

In view of my experience, I deny that weak acids must necessarily be formed in the sugar solutions by the use of Mr. Beanes' process, except under the most careless and unskillful management. Only two circumstances can give rise to the formation of weak acids in connection with the process of Mr. Beanes. Either the bone-black is left acid and the liquor dissolves acid from the bone-black, or the water left in the bone-black weakens the liquor sufficiently to allow fermentation to take place readily.

At the refinery of Messrs. Havemeyers & Elder there is no trouble

from either cause of acidity. The sugar solutions after filtration are neutral.

I am led to believe, from the passage of Dr. Wallace's discourse quoted above, that an account of the means we employ to avoid acidity in the bone-black, would be of use to the refiners in England, who purify their bone-black by Mr. Beanes' process. I may briefly state that acidity is prevented in the bone black—

1. By using peroxide of manganese in the hydrochloric acid generator, to avoid the formation of sulphurous gas.
2. By saturating the bone-black with hydrochloric acid gas only when it is dry and very hot, as it comes from the revivifying kilns.
3. By using the hydrochloric acid gas dry.
4. By allowing the saturated bone-black to stand in suitable receivers till the excess of gas, if any, is absorbed before washing.
5. By washing the bone-black thoroughly after saturation to remove the chloride of calcium and other soluble salts.

It may be well to state that the use of wet bone-black in the filters consequent on the employment of the process of Mr. Beanes, is not of itself a cause of acidity in a refinery. On the continent of Europe, and I believe in England, the bone-black is often used wet in the filters without any harm resulting from this practice. Differences of results obtained from the use of wet black must be due to differences of management.

In the two largest refineries in the United States, that of Messrs. Havemeyers and Elder of New York, and the Franklin Sugar Refinery of Philadelphia, also at Las Canas, the sugar estate of Don Juan Poey, the scientific planter of the island of Cuba, the process of Mr. Beanes has been adopted, is working successfully, and the best results have been obtained.

Dr. Wallace renders a just tribute to the process of Mr. Beanes, when he says that it is "otherwise all that would be desired." I am able to add that it is not necessarily a source of acidity in a refinery.

The apparatus in use at the above-mentioned establishments differs from the one previously in use in the manner of drying the hydrochloric acid gas, which is accomplished without the use of chloride of calcium.

Hoping that the above may prove useful to the sugar refiners of England,—I am, etc.,

P. Casamajor.

98 Wall St., New York, Feb. 22, 1869.

To this appeared next week the following commentary by Mr. Beanes.

To the Editor of the *Chemical News*.

Sir:—Will you kindly afford space to explain the cause of the apparent inconsistency between the views expressed by Dr. Wallace

on the above subject, and those which Mr. P. Casamajor has embodied in the letter which you published last week?

The fact announced by Dr. Wallace, that "acids invert cane sugar," has been known to me for the last thirty years, and is, I believe, familiar to most persons engaged in the sugar manufacture. So well, indeed, is it understood in America and on the continent of Europe, that every precaution is taken to neutralize the acidity of the crude sugar before submitting it to the action of the animal charcoal. This practice does not prevail in England; sugar refiners here trust to the calcic carbonate present as an impurity in the charcoal for the neutralization of the acid of the sugar; and, as my process removes this calcic carbonate, it renders the charcoal less suitable for the rude and unscientific method in vogue here, than it is for the greatly superior method adopted in countries where the manufacture is better understood.

The decolorizing effect of bone-black upon syrup is in proportion to the extent of *carbon surface* exposed. The smaller the grains the greater is the decolorizing effect produced; and yet our sugar refiners load with calcic carbonate —*whitewash*, as it were—the very carbon surface upon which they depend for the decolorization of their syrups! The practice seems to me, I confess, irrational in the highest degree.

Old bone-black, freed from lime and other impurities, is inferior in power to new, because its carbon surface is less. Its innumerable little prominences have been removed by friction, it has become polished, and the extent of surface which it exposes is thereby reduced, but it is nevertheless, as might have been expected, very much more efficient than a similar black from which the calcic carbonate has not been removed. I am, etc.,

E. BEANES.

Cordwalles, Maidenhead, Berks,
March 15, 1869.

Which elicited the following reply:—

To the Editor of the *Chemical News.*

SIR:—In your issue of this week (*Am. Repr.* May, 1869, p. 259), there is a letter from Mr. E. Beanes, which seems to call for a brief reply from me.

Mr. Beanes makes it appear that I "announced" the fact that "acids invert cane sugar" as an original observation, and rejoices in having known it for the last thirty years. I need scarcely state that "the fact" is one of the first with which students of organic chemistry are made acquainted.

Mr. Beanes further states that "our refiners load with calcic carbonate the very carbon surface upon which they depend for the decolorization of their syrups!" This is, as applied to British refiners, simply absurd. The calcic carbonate, which is not an

impurity, as Mr. Beanes states, but an essential constituent of animal charcoal, continually decreases with use, and, occasionally, almost entirely disappears. I am, etc.,

William Wallace.

Glasgow, March 27, 1869.

A NEW ELEMENT ACCOMPANYING ZIRCONIUM, DISCOVERED BY MEANS OF SPECTRUM ANALYSIS.

At the *soirée* of the Royal Society, on Saturday last, March 6th, Mr. H. C. Sorby, F.R.S., exhibited for the first time some phenomena in his spectrum microscope, which have led him to the conclusion that they are due to the presence of a new element, for which he has proposed the name of jargonium. The following is the account which Mr. Sorby then gave of this discovery:—

"Jargonia is an earth closely allied to zirconia, existing in small quantity in zircons from various localities, but constituting the chief ingredient of some of the jargons from Ceylon. It is, however, distinguished from zirconia and all other known elementary substances by the following very remarkable properties. The natural silicate is almost, if not quite, colorless, and yet it gives a spectrum which shows above a dozen narrow black lines, much more distinct than even those characteristic of salts of didymium. When melted with borax it gives a glassy bead, clear and colorless both hot and cold, and no trace of absorption bands can be seen in the spectrum; but if the borax bead be saturated at a high temperature, and flamed, so that it may be filled with crystals of borate of jargonia, the spectrum shows four distinct absorption bands, unlike those due to any other known substance."

It appears, however, by the following communication from Professor A. H. Church, M.A., that a similar, if not identically the same, discovery was published nearly three years ago. Professor Church writes as follows:—

"Royal Agricultural College, March 6, 1869.

"I heard, when in London this week, that Mr. Sorby had discovered a set of black absorption bands in certain zircons, and had attributed their occurrence to the presence in the stones examined of a new element accompanying the zirconium. May I be permitted to refer your readers to the woodcut of these bands, which I published three years ago in the *Intellectual Observer* for May, 1866? The sketch was rough, and the cut badly executed, but it shows, though without the delicate shading of the original bands, the effect on solar light of its passage through a considerable thickness of a particular Ceylon zircon, or jargoon, in my possession. Not only did I describe the bands, but I noticed their occurrence

in the spectra of some stones from particular localities (Ceylon), and their absence from stones from other localities (Espailly). I also added my views as to the cause of these bands—the presence of an element in some specimens, not found in others. I quote one or two sentences from the note referred to above:—'I am induced to hazard the conjecture that it may be, after all, Svanberg's *norium*, which determines the difference.' 'The absorption bands of zircon resemble those of didymium discovered by Gladstone, in their sharpness, and in their being produced by the passage of light through a colorless medium.'

"Since 1866, I have worked at intervals on the subject of the supposed norium. The rarity and costliness of the best and purest materials for the research—viz., transparent and flawless, and nearly colorless specimens—have seriously retarded the progress of my research. I have nothing ready for publication, but simply note the following points, which I have established with more or less certainty since the first conjecture which I made of an element different from zirconium in the 'black-banded' jargoons:—

"1. That zirconium is accompanied by at least one other metal (probably by two), in some of the zircons from Indian, American, and Norwegian localities.

"2. That the seven black absorption bands discovered in 1866 are characteristic of this new element, in some of its combinations at least, although I have not traced them definitely in any solution as yet.

"3. That the atomic weight of one of the new accompanying elements is different from that of zirconium.

"4. That the density of those zircons which show the black bands most intensely in the smallest thickness, is lower than that of those zircons which show no bands or only very faint bands. (In 1868, I showed that neither the direction in which the light was transmitted through the crystal, nor the action of an intense ignition, had any apparent influence on the particular absorptive power of the zircons for light now under discussion.) Of course comparison must be made with specimens in their native state and without flaws, or with specimens which have been similarly ignited, by which process the density is increased.

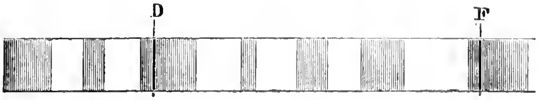
"5. The characteristic salts obtained from the black-band zircons may prove to have been included under those of norium by Svanberg. But as that term does not include some bodies which are certainly different from those which seem to belong only to the black-band zircons, a new name may have to be coined. *Nigrium* suggests itself as at once appropriate and consistent with the received system of nomenclature.

"I may add that the black bands were shown or described at the time of their discovery to many of my friends, among whom I may name Dr. Gladstone, Mr. Slack, and Mr. John Browning.

"Some of the zircons employed in my experiments are described

in the *Chemical Society's Journal* for 1864 (November and December). The best was of a very pale greenish hue and without a flaw. It weighed 1.1665 grammes, and its density before ignition was 4.579, after 4.625. A rather smaller Espailly specimen was orange-red before ignition, and had a density of 4.863. After ignition it became colorless, but its density remained the same. In neither state did it exhibit any black bands, nor have I obtained from several ounces of the zircon from this locality any other salts than those of zirconia."

Professor Church has forwarded the original drawing of the bands, torn out of his note-book. From this the following woodcut has been engraved.



It appears only fair both to Professor Church and Mr. Sorby, that the original article in the *Intellectual Observer* for May, 1866, should be given together with the above more recent notes. We therefore reprint it as follows:—

"MICRO-SPECTROSCOPE INVESTIGATIONS.

"*Letter from Professor Church.*

"The Editor has received the following interesting letter from Professor Church:—

"'Have you tried the experiment with chloride of cobalt, which I mentioned to you? If you take the saturated cold solution of this salt, it will give the spectrum roughly sketched in Fig. 1,* a *thinner* film of the *same* solution, heated (on a glass slide with thin cover) over the candle or lamp, gives the spectrum drawn in Fig. 2†. You will notice two black bands, I had almost said lines, in the red. As might be predicted from the change of color on heating, the solution is afterwards much more transparent to rays beyond D. The chlorides of copper and nickel also give very interesting results.

"But I think you will be most pleased with the experiment I have now to relate. I have worked lately on the spectra of pleochroic minerals and salts. Among the minerals recently examined were several fine specimens of the true zircon or jargoon, a silicate of zirconia. These gave a beautiful and most characteristic system of seven dark bands quite different from those belonging to any other substance

* "The figure alluded to, shows the red darkened, the orange light, and a broad dark band commencing to the right of the yellow and extending beyond the line F; the remainder of the spectrum is cloudy."

† "Fig. 2 shows the narrow black bands in the red, modified tints replacing the broad dark band of Fig. 1, the blue coming out clear. The experiment is a very beautiful one."

yet examined. They are roughly sketched in the following figure.* Zircons as colorless as common glass, show these bands as well, perhaps better, than those possessed of color. They are to be observed with zircons which have been ignited, as well as with those still in their natural condition. But some zircons show the phenomenon better than others, this difference not being due apparently to the color of the stone or the thickness through which the light traverses. I am not quite sure, but I incline to think that those zircons which have come from some localities show the bands better than those from others. Several Espailly specimens scarcely exhibit anything of this kind; all those from Ceylon and Norway show the bands well. From this observation I am induced to hazard the conjecture that it may be, after all, the presence of Svanberg's *norium* which determines the difference. You are aware that the orange jacinth, a variety of zircon, is very precious, and that the essonite, or cinnamon-garnet, is constantly sold for it. Curiously enough, the cinnamon-garnet, or essonite (a lime-garnet), has no conspicuous dark absorption bands at all, and so the spectroscope may be brought to bear upon the discrimination of these two stones. We have thus a much more ready process than that of taking the density of the specimens. The lime-garnet is of comparatively small value. The iron-garnet of different shades (carbuncle almondine, etc.,) gives a beautiful and very characteristic spectrum with several intensely deep absorption bands.

" 'I write these particulars of my experiments at once, for I thought you might like to make a little paragraph about them for the readers of the *Intellectual Observer*.

" 'I ought to add that the absorption bands of zircon resemble those of didymium, discovered by Gladstone, in their sharpness and in their being produced by the passage of light through a colorless medium. Silica, the other constituent of zircon, gives no bands.' "

The first who appears to have published researches on this subject is Svanberg. From his experiments, he came to the conclusion that zirconia was not a simple earth, but a mixture of three, or perhaps even a greater number, of metallic oxides; and that these oxides are present in different proportions in the zircons obtained from different localities (*e.g.*, Siberia, Norway and Ceylon), and in the hyacinth from Espailly in France. The atomic weight of these earths, (supposing them to be sequioxides) varies between 75 and 105·6, the mean of which, 91·2, is the atomic weight assigned by Berzelius to zirconia regarded as a simple earth. Svanberg could not succeed in completely separating these earths; but he found—1st. That the oxalate of one of them is less soluble in acids than the oxalates of the rest. 2d. That the chloride of the radical of one of the earths is less easily soluble in hydrochloric acid than the corresponding compounds of the other radicals. 3d. That the sulphate of one of them, when mixed with a large quantity of

* "The diagram referred to is substantially the same as the copy from Professor Church's drawing, printed above."

free sulphuric acid, crystallizes much more easily than the sulphates of the rest, and likewise in a peculiar form. To the earth thus distinguished from the others with which it is associated, Svanberg gave the name of noria. This earth is likewise found in zircons from the Ilmengeberg. In the eudialyte from Greenland, Svanberg thought he had discovered (besides cerium lanthanum, and didymium) two other earths, the first of which closely resembles yttria; the second has a yellow color. According to Watt's "Dictionary," article "Norium," a subsequent experimenter, Berlin, throws doubts upon the composite nature of the earth commonly called zirconia. Now that attention is again directed to this subject, it is to be hoped that these doubtful points will be cleared up. There seems no lack of new elements waiting to be discovered, and further researches may show that Svanberg's noria, Church's nigria, and Sorby's jargonia, are each separate entities.

Since the above was in type, we have received the following communication from Mr. Sorby:—

"36 Oxford Terrace, London, W. March 8, 1869.

"I send you an account of some of the objects I exhibited at the *soirée* of the Royal Society on Saturday, March 6th, illustrating the substance which gives such a remarkable spectrum.

"The specimen I showed was part of a jargon belonging to my friend, Mr. William Bragge, of Sheffield, who most kindly and liberally gave it to me for study. I soon found that the spectrum was not due to zirconia, since the zircons from some localities give no bands whatever; those from other localities show traces of the bands, as if they contained a small variable amount of the substance in question, mixed with zirconia, in the same manner that many are variously colored with small quantities of the oxides of iron. If the borax blowpipe bead of this substance had given absorption bands, in the same manner as the oxides of didymium, erbium, cobalt, and uranium, there would have been no difficulty in proving whether or no it was a new elementary body; but when melted with borax it gave a clear, colorless bead, when both hot and cold, without any trace of absorption bands. Very many known oxides give such beads, and the question was whether any of these would give the remarkable absorption bands when they were in a crystalline state.

"There was no difficulty in proving that the crystalline silicates of a number of known earths and metallic oxides do not give any bands, but still a number of elementary substances could not be procured in combination with silica, or not in crystals sufficiently transparent to enable me to ascertain the fact for certain, though I prepared a large series of sections for this especial purpose. It was therefore open to doubt whether the substance which gave the bands was a new one or not, though I proved by my new blowpipe method that it was very closely allied to zirconia.

"At length it occurred to me, that perhaps the production of the absorption bands depended on the substance being in a *crystalline* state, and I therefore saturated a borax blowpipe bead with it, and by flaming caused it to become full of minute crystals of the borate, so as to be white and almost opaque. By using a strong illumination, I succeeded in causing light to pass through the bead, and found that the spectrum then showed four absorption bands, very well marked, considering the nature of the case. None of these bands corresponded exactly with those of the silicate, and when microcosmic salt was added, so as to give rise to crystals of the phosphate, a spectrum was obtained with bands differing from both the others. The fact of the clear borax bead showing no bands until it is filled with crystals is, in my opinion, the most important yet discovered in connection with the subject, for it enables us to prove that the substance which gives the bands is no known earth or metallic oxide. Since it therefore appeared to be a new substance, and was found in certain specimens of the *jargon*, I thought no better name could be adopted than that of *jargonia*.

"This was the state of my knowledge of the subject when I came to London a few weeks ago, bringing with me the printed description of most of these facts, to give away at the *soirée* of the Royal Society. My attention was subsequently called to Professor Church's letter in the *Intellectual Observer* for 1866, p. 291, which I had not previously heard of, since that work is almost unknown in the northern provinces. Judging from his description and figure, there can be no doubt that he had observed the spectrum of the same substance, only comparatively in a very imperfect manner, as will be seen by comparing his figure with that copied from my own drawing, shown at the Royal Society. He speaks of 'seven dark bands,' whereas my specimen shows double that number. He also says that 'all those from Ceylon and Norway show the bands well.' Now after having examined several hundred jargons from Ceylon, I must say that, in comparison with my own specimen, scarcely any show the bands *well*. There is not one single specimen in the British Museum that shows them even moderately well. One in the Museum of Practical Geology shows them in a very satisfactory manner, and I have seen two or three others that show them, though not anything like so well as my own, the exact locality of which must be considered to be still unknown for certain. Those from Norway show the bands in the same comparatively imperfect manner as the usual specimens from Ceylon, and, even assuming that my specimen contains but little zirconia, we must conclude that the zircons from both those localities contain only a small amount of the substance for which I have proposed the name *jargonia*. If any one had only seen the usual kind of specimens from Ceylon or Norway, I can easily believe that he might have been led to conclude, with Professor Church, that the production of the bands might depend on the presence of Svanberg's *noria*. However, since

the zircons from Norway (Frederikswarn), which, according to Svanberg, are so rich in his noria that it materially modifies the chemical equivalent of the earth and the specific gravity of the mineral, are, according to my own spectroscopic observations, so very poor in my jargonia that they give only a very faint trace of the bands, I cannot admit that they are the same substance. My friend, Mr. David Forbes, has most kindly allowed me to examine the zircons from many localities in Norway, collected by himself; and given me a number to cut for examination; and I find that none contain more than a decided trace of my jargonia.

"My own specimen of jargon is so rich in jargonia that a piece one-tenth of an inch in thickness, and all but colorless, gives fourteen bands, as shown in the accompanying figure. Unlike most other absorption bands, thirteen of these are narrow and perfectly black lines, surpassing in this respect even those characteristic of

salts of didymium. When the section is cut parallel to the axis of the crystal, though it is almost colorless with ordinary light, it is slightly dichroic, giving a reddish and greenish image. The reason of this is easily understood when the spectrum is examined with a double image prism, so as to give two spectra side by side with the light polarised in opposite planes, as shown in the accompanying figure. The image in which the light is polarised in a plane parallel to the axis of the crystal (No. 1) show a well marked double band in the red which is absent from the other image (No. 2), whilst that shows a band in the yellow part of the green which is absent in the other. There is also a difference in the position of several of the bands, and a difference in the intensity of others, whereas that in the yellow is nearly the same in both. These are remarkable peculiarities, since in most dichroic substances there is merely a difference in the intensity of all the bands.

"On the whole, therefore, it seems to me that these very striking absorption *lines* are due to an elementary substance not hitherto recognized, which can crystallize in all proportions as a silicate along with zirconia, and whose action on the spectrum varies to an unusual extent, according as it is in a vitreous state or crystallized in combination with different acids."

THE SALT DEPOSITS AT STASSFURT.

By Messrs. BALD AND MACTEAR.

(Concluded from page 343.)

WE will now consider the beds *seriatim*, beginning with the lowest, which is called the *anhydrite region*, and consists of 685 feet of pure rock salt interspersed with thin layers of anhydrite a quarter of an inch or so thick, and dividing the salt at intervals of from one to seven or eight inches. The salt is pure and colorless when pulverized.

The anhydrite is anhydrous sulphate of calcium, and contains a small quantity of a bituminous matter which imparts to it its char grey color; traces of organic remains are also proved by the presence of a gas containing carburetted hydrogen which, according to Bischof, has the following composition:—

Carburetted hydrogen	85
Carbonic acid	3
Atmospheric air	12
	100

It is present in quantities of about 3 c.c. per kilogramme in the rock salt and about 8 c.c. in the kali salts. It presents the appearance of air bubbles in the transparent crystals. The specific gravity of anhydrite is 2·968, and it is soluble in water to the extent of 1 part in 500.

In the second or *polyhalite region* we have, besides the common salt and sulphate of lime, a deposition, from what might be considered the mother liquors, of the sulphates of potassium and magnesium which have combined with the sulphate of calcium to form the salt polyhalite, from which this division takes its name; its composition is given on the table:—

$$_2CaSO_4 + MgSO_4 + K_2SO_4 + 2H_2$$

It has a specific gravity of 2·720, and is immediately decomposed by water. Specimens of it are seldom found pure, as they generally contain from 2 to 6 per cent. of chloride of sodium. This bed is about 200 feet thick, and has an average composition, according to Steinbeck, of—

Chloride of sodium	91·20
Anhydrite	0·66
Polyhalite	6·33
Hydrated chloride of magnesium	1·51

the upper layers, however, being the more impure.

In the third *kieserite region* the gradual disappearance of the more insoluble salts is made manifest, for it contains on an average only 2 per cent. of anhydrite, and about 60 per cent. of common salt, and from 17 to 20 per cent. of kieserite, which is monohydrated sulphate of magnesium, the former being — $MgSO_4 + H_2O$. Specimens found in the mine generally contain from 1 to 2 per cent. of chloride of sodium or magnesium; it is amorphous, greyish-white, and transparent, and in the air has a tendency to pass into epsoms, becoming opaque during the transformation; it is soluble in rather more than twice its weight in water (40·9 parts in 100 H_2O). When the quantity of water is not sufficient for complete solution, this salt has the peculiar property of absorbing a certain quantity of it and setting into a hard mass, more resembling a piece of flint than anything else, and with, of course, a considerable increase of volume.

In the fourth and last division, which is called the *carnallite region*, the insoluble salts are entirely gone, and even the common salt gives place in quantity to the more soluble carnallite, the average composition being:—

Carnallite	55
Common salt	25
Kieserite	16
Hydrated chloride of magnesium	4

Carnallite, when pure, consists of $KCl + MgCl_2 + 6\,H_2O$, having a specific gravity of 1·618, and dissolving in about one and a-half times its weight in water at 18° C.; it is crystalline, clear and colorless, but as found in the mine it varies from pure white to a deep red color, owing to the presence of minute quantities of peroxide of iron. This peroxide of iron, when separated from the salts, presents the appearance of a coppery bronze powder, but when viewed under the microscope it is found to consist of distinct crystals of exceedingly beautiful appearance, varying in color from golden yellow to dark red.

The carnallite is very deliquescent, and, on exposure to a damp atmosphere, the chloride of magnesium gradually drains away, leaving the chloride of potassium behind. This probably accounts for the presence of sylvin, or pure chloride of potassium, small quantities of which are found underneath the carnallite; it is rather more abundant in the Anhalt mine, and this would further tend to prove the theory that it is the product of the decomposion of car-

nallite, as the chloride of magnesium is found to preponderate in the lower lying level of the Stassfurt mine as *tachydrite.*

Sylvin is variously colored, has a bright, shining appearance, which has been not inaptly compared to mother-o'-pearl. Its specific gravity is 2·025, and 34·5 parts of it dissolve in 100 of water at $18\frac{3}{4}$° C.

It is occasionally found in large perfectly transparent crystals, which, according to Professor S. Magnus, of Berlin, are as transparent to heat as rock salt; and this diathermic property does not change with the temperature of the source of heat any more than rock salt does, which has hitherto been the only substance known to possess the latter quality.

The tachydrite already mentioned is a salt having the same composition as carnallite, but in which the potassium is replaced by calcium, its formula being $Ca\,Cl_2 + 2\,Mg\,Cl_2 + 12\,H_2O$. It is very deliquescent and very soluble, 100 parts of water at $18\frac{3}{4}$° C. dissolving 160·3 parts of the salt; it is the only salt which raises the temperature of the water during solution, all the others having the property of lowering it during that operation.

Besides sylvin and tachydrite, there exists also, though in such irregular quantities that it cannot be calculated upon with certainty, a salt called kianite, having the following composition: $Mg\,S\,O_4 + K_2\,S\,O_4 + H_2O$. It is evidently the product of a secondary decomposition, arising from the action, probably, during a low temperature, of sulphate of magnesium upon chloride of potassium.

We also have what is probably the most peculiar and unaccountable compound in these mines, viz: boracite, the composition of which is $6\,Mg\,O, 8\,B_2O_3 + Mg\,Cl_2$ (sp. gr. 2·3); it is found scattered all over the deposit in nodules varying in size from the most minute up to 7 or 8 inches in diameter, and, although occurring in the most soluble salt beds, is of itself almost insoluble in water, and, in fact, is with difficulty decomposed by acids; but its greatest peculiarity is, that it always, and without exception, contains a kernel of the easily soluble carnallite or tachydrite; it does not exist in any great quantity, the annual yield being somewhere about 10 tons. A small quantity of bromine is also found in this region, existing as bromide of magnesium; cæsium and rubidium can also be detected; but hitherto all attempts to prove the presence of either lithium or iodine have been without success.

The ingredients in this region do not exist as a homogeneous mass, but are deposited in distinct layers, which repeat themselves frequently, and vary in thickness from a mere line to several feet.

It is from the impure carnallite that the manufacture of muriate of potash is so largely carried on in the neighborhood. This salt, which is coarsely ground at the mines, has an average composition of—

Chloride of potassium	16
Chloride of magnesium	20
Chloride of sodium	25
Sulphate of magnesia	10
Water	29
	100

It also contains small quantities of sulphate of lime and bituminous matter, which occasion the manufacturer some considerable trouble, as in strong solutions they are light and flocculent, and, consequently, somewhat difficult to settle.

The manufacture of the muriate is entirely a question of the solubilities of the various salts, the key to which is the fact that the double salt of chloride of potassium and magnesium forms only from solutions containing exactly double the quantity of chloride of magnesium which exists in the carnallite. You will find it stated by various authorities that the "carnallite crystallizes only from solutions containing a large excess of chloride of magnesium;" but this has been proven by experiment to be a definite chemical quantity, viz: 4 parts of chloride of magnesium, and 1 part of chloride of potassium—or, in other words, 2 parts of chloride of magnesium, hold in solution up to a certain strength 1 part of carnallite. So that on dissolving the crude salt in water, the chloride of magnesium takes up its quantity of chloride of potassium, whilst the remainder crystallizes out as muriate, mixed with common salt and a small quantity of sulphate of magnesia. The mother liquors are then further boiled down to obtain a crop of artificial carnallite, which, in turn, is treated in a similar manner to the raw salt, to obtain a similar supply of muriate. The muriates produced vary in strength from 75 to 98 per cent.

Epsom salts are also prepared at some of the works from kieserite, whilst at others a considerable quantity of the double sulphate of magnesium and potassium, a compound containing one equivalent of each of the sulphates, combined with 6 atoms of water, is made.

This salt is largely used as a manure for the sugar beet. It is generally understood that the beet grows equally well with soda as with potash, but the cultivators prefer to use the latter, as it is nearly all recovered in the state of carbonate, and, of course, is greatly enhanced in value. This leads us to consider what would have been the present state of the potash trade had it not been for the opportune discovery of this deposit, previous to which our only sources of potash were the muriate and sulphate from kelp, principally wrought in Glasgow and the North of Ireland, and the carbonate or potashes of North America. Whether the supply has regulated the demand or not, it is difficult to say; but it is a fact that the produce from the two last-named sources has rather increased than decreased, whilst we have in addition the large supply obtained from the Stassfurt deposits.

This, of course, has been followed by a corresponding reduction in price: muriates of 80 per cent. which, in 1866, sold at £21 10s. per ton, can now be purchased for £8 10s.

The carbonate has not fallen in the same ratio, owing to the increasing employment of it in the arts and manufactures.

There is, consequently, a great prize in store for the chemist who, by his ingenuity, can discover some more direct process than that at present in use for the conversion into carbonate of the vast quantities of chloride stored up in these German mines.

To the scientific chemist and geologist this deposit presents a vast field well worthy of attentive study and research.

The generally accepted theory is that it is the product of the slow evaporation of some vast ocean, by a process similar to that which is at present going on in the Dead Sea, the waters of which are supposed to have already been evaporated down to 1·300 from their original level. If we examine an analysis of this sea, we find that it contains $6\frac{1}{2}$ per cent. of chloride of sodium, $1\frac{1}{3}$ of chloride of potassium, $2\frac{3}{4}$ of chloride of calcium, $10\frac{1}{2}$ of chloride of magnesium, and about $\frac{1}{4}$ of bromide of magnesium. The sulphates have almost entirely disappeared; and, looking at the preponderance of the more soluble salts over the chloride of sodium, we are forced to the conclusion that there must be already deposited at its bottom a vast quantity of the latter salt.

The great Salt Lake of North America, some others in the south of Russia and in Asia, which contain almost nothing but chloride of sodium, must be regarded as fresh water lakes, which derive their saline matter from some already formed deposits of salts.

In August, 1867, the Prussian government commenced boring for salt at Sperenberg, and at the end of August, last year (1868), they had penetrated to a depth of 952 feet, principally through gypsum, when they suspended operations to admit of more powerful instruments and machinery being made. These being now supplied, the work is again going on. There is no doubt that they will reach the salt strata, and we await the result with considerable interest, to see if the potash salts exist there also.

There are others besides scientific men to whom these mines are a source of considerable interest. To the political economist they mean an almost inexhaustible supply of the "savor of the earth," employment to the people, trade to the country, and pounds, shillings, and pence to the merchant and manufacturer.

To the visitor, be he scientific or non-scientific, a visit to the mines will amply repay him for his trouble.

The appearance of the workings in the kali salt portion of the mine is no less beautiful than wonderful, and is so entirely unlike what we see in the mines in this country, that we are entirely at a loss for anything to which we can compare them, and it is utterly impossible for any description to prepare the visitor for the novel

sight which meets his eye when he enters these workings for the first time.

All have heard of, and many have witnessed, the wonderful grandeur of the Mammoth and other caves of North America, with their unfathomable subterranean rivers, where size and form, together with the light and shade produced by the flickering torches of the guides, are what excites the admiration of the traveller.

At Stassfurt we have neither the rivers nor the vast size, but we have space, which, in the bowels of the earth, seems great; we have beautiful form in the sparkling irregular angles formed by the pick and in the cavities left by the blast, to enhance which there is the magic charm of color—colors of nearly every tint in the rainbow, passing from a deep purple through crimson, bright red, orange, and yellow, to a snowy white—all in regular layers, but varying in size and arrangement, and from the angle of the dip each layer forming an almost perfect arch. The effect is further heightened by the nodules of boracite, which have the appearance (as graphically described by a Glasgow gentleman who is well acquainted with the mine) of having been shot at random from a park of artillery, so irregularly are they scattered, and so firmly are they imbedded in what meanwhile seems to us an altogether foreign place for them.

On the occasion of our visit, after having been conducted through such a gallery as we have attempted to describe, we left the main working, and after going a few yards through a narrower passage we found ourselves in a small chamber about twenty feet square—the preliminary opening for a new working. Underneath what is now the roof of this cavern there had been a lodgement of water, which had completely dissolved all the more soluble salts, and left the common salt and chloride of potassium in magnificent crystals of absolute purity; this formed a dome-shaped roof, which sparkling and glistening in the light of our lamps, rivalled in beauty anything we had ever imagined of the celebrated Valley of Diamonds.

The large Reflecting Telescope, to purchase which the Legislature of the English colony of Victoria appropriated $25,000 gold, has arrived at Melbourne, the capital, and is hereafter to be known as the Great Melbourne Telescope. The Government Astronomer in Victoria reports that a rectangular building, 80 by 40 feet, with a traveling roof, is in course of erection, to lodge the instrument. The colonial Legislature has further appropriated $8,500 to pay for the building. The piers on which the instrument will be supported are to be constructed of solid blocks of gray basalt, weighing from one to three tons each. When this magnificent instrument is mounted, the nebulæ of the southern heavens will be examined, and the results will greatly advance the progress of astronomical science.

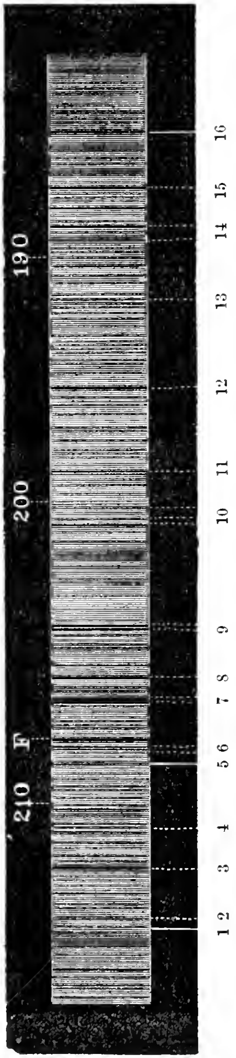
EDUCATIONAL

SUNLIGHT AND MOONLIGHT.

By Prof. Henry Morton, Ph. D.

(Concluded from page 347.)

Such, then, are the chief phenomena presented to our view in an attentive examination of the sun, with reference to its constitution and condition.



But one point more remains to be considered in this relation, namely, the facts revealed to us by spectrum analysis of the sun's general light.

When sunlight, which has traversed a number of prisms properly arranged, is caused to impress an image upon the eye or a sensitive plate, a band or spectrum is produced, crossed by a multitude of parallel dark lines, of very irregular width and intensity, such as are shown in the accompanying figure, which represents about one-eighth part of a photograph of the solar spectrum made by Mr. L. M. Rutherfurd.

It was first shown by Kirchhoff, that these lines were probably the result of electric absorption exerted upon the light of the solar photosphere by layers of gas resting above it. He showed, in fact, that by causing the light of a gas flame which produces a continuous spectrum of colors, without any such dark lines, to pass through various gases and vapors, similar bands were developed. Such an experiment may even be repeated on the large scale.

Thus, I will here, with this lime light, these prisms and lenses, develop upon that screen a brilliant spectrum of some ten feet in length, made up of a continuous band of blended colors, running from red at one side through orange, yellow, green and blue, to violet at the other.

By now vaporizing in the path of that beam a globule of the metal sodium, I cause, as you see, a strong black line to appear in the orange part of the spectrum, while all the other colors remain as clear as before, and this black line fades out when the vapor has dissipated.*

It would, then, seem highly probable that if such a line as this sodium vapor produces is found ready-made in sunlight, the sunlight has traversed a mass of sodium vapor. So likewise with other elements. By a comparison of the lines producible by various known elements with those found in the solar spectrum, the presence of various elements in the atmosphere of the sun has been demonstrated.†

We have thus identified Hydrogen, Sodium, Iron, Magnesium, Calcium, Copper, Cobalt, Barium, Nickel; while on the other hand we fail to find the lines which are produced by Mercury, Lithium, Copper, Silver, Zinc, Tin, Lead, Antimony, Alluminium, Cadmium, Arsenic, Strontium, Chromium, Lithium, Rubidium, &c.

Thus in Fig., line F. corresponds with the principal band produced by Hydrogen, lines 6, 7, 8, 9, 10, 12 and 15, with some of those given by Iron, line 8, with one of Calcium, line 4, with one of Cobalt, line 1, which is very faint, with one of Gold, 5, 13, and 16 with Nickel, and 11 with Barium. The two lines, 14, indicate Air.

On the other hand, in the place indicated by 2, there should be a line, if Cadmium were present; and the strong double line at 3 has not, we believe, been identified with that of any substance.

Thus we have attained to a chemical analysis of the Sun, more than ninety millions of miles away; and it is, moreover, interesting to note, that when the same process is applied to the light of the fixed stars it reveals to us a similarity and yet diversity in their composition. Thus α Orion appears to lack Hydrogen, Tellurium, Antimony and Mercury, which are indicated in Aldebaran, and except the first, are wanting in the Sun.

* The details of this experiment will be found described in this *Journal*, Vol. LI., p. 420. We should, however, as the result of further experiment, modify a statement there made in connection with this subject, by saying, that with certain kinds of glass, the flame *above* the rod is the *best* part for showing the *luminous* sodium line.

† We have omitted all reference to the relation between emission and absorption in this place, as not essential to the subject in hand, and involving too elaborate an explanation. In a detailed discussion of spectrum analysis, which will be published in subsequent numbers, the whole subject will be treated.

Iron, it is to be observed, is one of the prominent constituents of the solar atmosphere, as thus studied; and it may seem strange that such a body should be thus found in a state of vapor. But that it is capable of vaporization, I shall presently show you.

[The speaker here placed himself, with a large hydrogen generator, such as is described in this *Journal*, Vol. LIII., p. 204, and an iron reservoir charged with compressed oxygen, as well as an oxohydrogen blow-pipe with concentric nozzles, upon a platform built on one of the stage-traps, and was raised about ten feet above the floor. Lighting the jet, and holding before it a bar of stėel, this was speedily melted, and even caused to boil so that scintillating particles were thrown in all directions, and a fountain of sparks and drops of molten steel fell from the point of the jet to the stage, thirteen feet below it, and rolled down into the sunken footlights, with an effect which was most impressive.]

The scintillation of this steel is an evidence to you of its vaporization, since it could only be by such a boiling of the melted metal that these fragments could be so widely dispersed; but we have, of course, many other evidences of the same thing, which, though more conclusive, are not adapted to exhibition on so large a scale.

Notwithstanding the absorptive action exerted by various vapors on the sun's light, this contains, essentially, all colors; the literal fact being, that while some particular shades or tints are absent or deficient, as shown in the *dark* lines, (these are not all black, but of various depths,) those at each side, which are so nearly alike as to be undistinguishable by the eye, do duty for the missing ones.

This fact of the composite character of white light, such as that which comes from the sun, may be illustrated in two ways—by analysis and synthesis. The demonstration by analysis has just been given, when the white lime light was resolved into a spectrum of all the colors. The demonstration by synthesis will be now shown. We have caused to be placed in this lantern at my side a wheel of glass with six segments of the principal prismatic colors, and, as you see on the screen, there appears an immense wheel thirty feet in diameter, having similar divisions.

We might indeed say that six threads or cords of colored light reached out from the lantern to the screen. I now rotate this little glass wheel, and so, as it were, spin into one great cord these variously colored threads. It is then, as you see, a cord of white light. This method of describing the experiment is, of course, merely

metaphorical, since light-rays are not substantial, and are incapable of a mechanical treatment such as we name. The actual facts upon which this result depends are those known as the phenomena of Persistence of Vision.

Briefly stated, they are these. Any impression produced upon the eye leaves a reverberation of itself for a certain time after it has ceased; if, then, another impression comes upon the eye before this echo of the first has died out, the two will be mingled and combined. So, also, if a number of impressions follow each other with sufficient rapidity, they will all be blended together by this action. Thus it it is in this case; each point of the disc to which the eye is directed presents, in rapid succession, each of the six colors as they rotate, so that before the impression of the first one has faded out all the others have been added to and combined with it. It is to the same action that we owe the very curious effects developed by the different forms of apparatus, which may be well classed together under the name applied to that first devised, and called "thaumatropes.' One of these fitted for exhibition on such a scale as this house requires I now bring before you.

You see a disc six feet in diameter and decorated with a brightly colored pattern of balls, rings, and star-points, to which is given a rapid rotation until the figures are blurred, confused and undistinguishable, when seen by the steady bright light issuing from this oxohydrogen lantern.

By means of a disc of card, with narrow slots cut in it, revolved in front of the lantern, we now break up the light into a series of rapidly recurring flashes. These flashes follow each other so closely that, by reason of the "persistent" action of the eye, just explained, the light seems to be as steady and uninterrupted as before. But how strange an effect is produced upon the rotating disc. The pattern is no longer blurred and confused, but each object is perfectly distinct, only they have all acquired life and motion. The rings rotate, the balls fly bounding through them, and the star shoots out and retracts its rays.

The reason of this is as follows:—The various parts of the design on the card have a "progressive" relation to each other. Thus, the first point of the star being small, the second is a little larger, the third larger yet, and so on to the sixth and longest, after which they diminish, so that numbers 5 and 7 are alike 4 and 8, 3 and 9, 2 and 10, these latter being next to number 1 on either side.

Now, suppose that when the first flash of light comes, the first or smallest point is uppermost, and that at the second, point 2 has taken its place by the rotation of the disc. It will then seem to us that the point has *grown out*, since no interruption to our view will have been perceived between the impressions. If, then, the other points come in place at the instant when the succeeding flashes occur, the out-darting and retreat of the point will be fully expressed. Of course, the success of this experiment depends upon the timeing of the flashes with the motion of the disc.

Further illustrations of the same property were then made with an apparatus on the principle of Gassiot's star of unusual size. This consisted of a light wheel, five feet in diameter, on which were attached a number of Geissler tubes, having an aggregate length of twelve feet. While these were in rapid motion, flashes from an induction coil of the largest size (containing thirty miles of wire, and made by Mr. E, S. Ritchie, of Boston,) were passed through, producing the appearance of a star with constantly changing colored rays of electric light.

Having thus illustrated the composite character of sunlight by the two direct methods of analysis and synthesis, I will, in turn and finally, demonstrate the same thing by an indirect method.

The appearance of color in non-luminous objects depends upon the presence of the same color in the light by which they are illuminated. Thus, this piece of red cloth appears to you to be red, not because it is *producing* red light, or it would be visible in the dark, but because all the colors of the rainbow falling upon it in the white light which illuminates the stage, it absorbs all but the red, and reflects to your eyes that color alone.

This I will prove to you by employing pure colored lights on this brilliantly painted drop-curtain, when you will see that with each light that color only will show. [The drop-curtain, having upon it one of Mr. Russel Smith's most beautiful pictures, representing Lake Como, was here lowered, and, by means of the chromatic burners, which will be described in an appendix, was lit alternately with red and green light. With the red light, the red parts of the painting only showed color; the other tints appeared black, with the green light; the red and other colors were black, the green alone showing.]

While I have been making these last experiments, my assistants have arranged the stage for a final illustration of this same point

on a grand scale, and I will "ring up" the curtain and introduce this, which may be literally called a "transformation scene."

[The curtain here rising, displayed the stage set with a brilliantly colored palace interior, illuminated by three powerful lime lights in addition to the usual gas-burners. Six "chromatic burners," in their non-luminous condition, were also seen distributed around the stage.

At a signal from the lecturer, there then marched in a company of masked figures in brightly tinted costumes, and carrying banners with colored devices, emblematic of the prismatic colors, &c.

These having coutermarched and grouped themselves around the stage, at a signal, the white light was turned down, while the chromatic burners were caused to emit a flood of yellow light.

Instantly every trace of color disappeared from all objects, while the effect upon the complexion was such as to reduce the troupe of masks to a most grisly phalanx of exhumed corpses.

The amount of light thus produced by the six groups, containing together ninety individual burners, was so great as to fully illuminate the house, so reducing the audience to a concourse of like fearful phantoms.

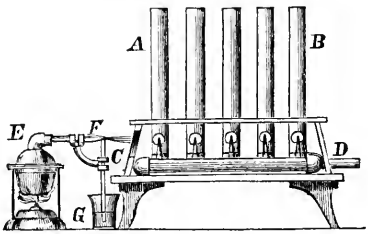
The aspect of this entire scene, this audience of spectres watching the performance of this group of ghosts, was a thing never to be forgotten by a spectator.

The white light was then restored, and everything flashed again into color and life.]

APPENDIX.

THE chromatic burners or apparatus devised by the lecturer for the production of monochromatic light, on this occasion, were arranged as follows:—

A gridiron-like frame of iron tubes was made, with five parallel bars united by cross-pieces so as to give a gas supply to all of them.



One of these parallel pipes is shown in the cut, at CD. From it rise five small pipes or jets, on each of which is placed a piece of iron pipe of ½-inch interior diameter, 12 inches high, and having two holes cut in it

very near the lower end, so that the short jets reach just above them.

These are held in place by short corks on the jets and by the cover of the box or case through which they pass. Ordinary burning gas being admitted by the pipe D, escapes through the twenty-five small jets into the twenty-five tubes, and, carrying with it a quantity of air, burns, when lit at the top of the tubes A B, with a pale-blue non-luminous mass of flame. We have here, in fact, a group of cheaply constructed Bunsen burners, all drawing their air supply from the interior of the box, whose only inlet is on the side above C. Opposite this opening is placed a small steam atomizer, consisting of a steam boiler E, (made of a tin cup with a cake-pan soldered on to the top, and a piece of copper or brass tubing inserted,) supported in a cylindrical tin stand over a lamp made by soldering two cake-pans together, with a bit of tube for the wick. The atomizer jet, F, consists of two glass tubes attached by a pewter support, which must be as unyielding as possible, to prevent loss of adjustment.

When steam is escaping freely from F, any liquid placed in G will be lifted and thrown, in fine spray, into the box.

To obtain colored light from these burners, it is therefore only necessary to fill G with a solution of some flame-coloring salt, such as chloride of strontium, or, much better, chloride of lithium, for red, copper or thallium for green, sodium for yellow, &c.

ON THE FUTURE DEVELOPMENT OF SCIENTIFIC EDUCATION IN AMERICA.

By S. Edward Warren, C. E.

Prof. of Descriptive Geometry, &c., in the Rensselaer Pol. Inst., Troy, N. Y.

(Continued from page 352.)

(*B.*)—*Descriptive Geometry*, or the geometry of form and position. This study we pause to mention, only briefly, as admirable in its power 1st, to promote clear and correct conceptions of *Forms* in space through inspection of figures (projections) quite unlike the forms themselves; 2d, to cultivate the habit of exactness and completeness of statements. Thus, two different plane sections, through the same point of a warped surface, will generally be curves, and convex in opposite directions at that point. Hence their tangents, at that point, will be on opposite sides of the surface. Their latter

lines, *being tangents at the same point*, their plane will be a tangent plane to the surface at that point. But, being on opposite sides of the surface, their plane must also be a secant plane. Hence, a tangent plane to a warped surface is, also, generally a secant plane.

3d. To enable one to discriminate between form and fact, appearance and reality, *the essential* and *the accidental*, failure to do which is the most mischievous, and, in many relations, the most sickening of all the exhibitions of human imbecility, either mental or moral. For the given magnitudes in a general problem may have an almost unlimited number of different relative positions, and thus its particular forms may often be in *appearance* very different. Yet identically the same method of solution may apply to them all, so that the same letters shall denote the essentially same points in all.

4. To stimulate fertility of invention. For the resources of the science are adequate to furnish the appropriate modifications of the general method for every possible special case of a real problem.

(*C.*)—*Language.* Of this it seems enough to say, that the close and critical study of the instrument of universal thought cannot but be a powerful means of mental discipline.

Painting expresses or embodies one class of thoughts; *Mechanism* another; Laws another; National Architecture another. Now, if earnest and protracted study of any of these is invigorating to the mind, on the ground of its being an expression of intelligence, how much more so must be the similar study of language as the expression of all intelligence and feeling.

Nothing seems clearer, then, on its face, than that the careful study of language, enthusiastically presided over and guided by the best teachers, should form a good share of the means of liberal culture. And, on the whole, it seems more important that *some* language should be thus studied, than that any particular one should be so pursued. To hint, most briefly, the reasons for this opinion: It has been thought that human life, now, is what it is by reason of the self-perpetual influence of that life in the past, without express efforts *now* to keep alive the peculiar elements of former life. But even if this be denied, and the question, *how* is the proper influence of the long-gone past to be perpetuated? be answered by saying that this must result from maintenance of the knowledge of the ancient languages, and of past customs and laws and life; yet, even then, *all* need not be engaged in the study of these things. The due influence of the past over the present will be effectively

secured through the living agency of a few earnest minds trained in the culture of the past, while in sympathy with the present. In this view, it seems to be happily demonstrated that each can be left free to follow his tastes, and the requirements of his intended pursuits, in the selection of the language through which he will get such training in precision of thought and speech, and, incidentally, in knowledge of other human life than his own, as the study of language can afford. For example, the ideal engineer should be inferior to none, in *gentle* as well as *solid* culture. Yet it would seem a piteous waste of time for him to read Virgil at the expense of omitting to read elegantly-written scientific works in French or German.

The inordinate homage paid by some to the Ancient Languages as an indispensable means of education and culture, compels the thoughtful in other directions to ask, In the name of Christianity and human nature, and for the credit of both, *why* can there not be a characteristic culture of the present, equal, in its kind, to that of the past, pagan as that was, and Christian as this should be? Has the race so degenerated for the past two thousand years, that there *can* be no original culture of the present equal to that of the past? Verily, the negative answer must betray an illusion—an illusion arising from this, that we are keenly sensible of the vices of the present, but pleasantly surprised by the amount of sound and elegant intellectual development attained in the past under an external civilization and religion far below those of the present.

(To be continued.)

Franklin Institute.

Proceedings of the Stated Monthly Meeting, April 21st, 1869.

The meeting was called to order with the Vice President, Mr. Coleman Sellers, in the Chair.

The minutes of the last meeting were read and approved.

The Actuary submitted the minutes of the Board of Managers, and reported the donations received at their stated meeting, held April 14th inst., from the Royal Astronomical Society, the Royal Geographical Society, and the Society of Arts, London, and the Association for Preventing the Explosion of Steam Boilers, Manchester, England; the Osterreichischen Ingenieur Verien, Vienna, Austria; la Société Industrielle, Mulhouse, France; the Natural History Society, Montreal, Canada; the Young Men's Association,

Milwaukee, Wisconsin; the Smithsonian Institution, Washington, D. C., Messrs. Charles H. Hart and Frederick Fraley, and the Board of Health of Philadelphia.

On motion, it was

Resolved, That the Board of Managers recommend to the Institute to adopt the following sections to be inserted in the By-Laws of the Institute as Article X., and to re-number the subsequent articles to correspond—

Article X.—"Organization and Government of Sections," &c. [See printed copy of By-Laws.]

On motion, it was

Resolved, That the application of the gentlemen whose names are thereto annexed, to be constituted the section of "Mechanical Engineers" of the Franklin Institute be approved, and is hereby ordered to be so reported to the Institute at their next meeting.

Names of Applicants.—J. Vaughan Merrick, Coleman Sellers, William Sellers, Washington Jones, Henry G. Morris, W. Barnet Le Van, Wm. B. Bement, Jos. M. Wilson, Jacob Naylor, Henry Cartwright, Robt. Briggs, John H. Towne, B. H. Moore, Enoch Lewis.

The regular report of the Resident Secretary was then read, and Mr. Thomas Shaw described his Gunpowder Hammer, or Artillery Forge.

The names of those appointed to serve on the various committees for the ensuing year were then read as follows:

Library.

Percival Roberts,
Samuel Sartain,
Charles Bullock,
Caleb S. Hallowell,
Pliny E. Chase,
B. H. Moore,
James S. Whitney,
John Redfield,
Henry Morton,
Henry W. Bartoll.

Arts and Manufactures.

William Adamson,
John H. Cooper,
Chas. G. Crane,
Henry R. Lawrence,
C. Eugene Meyer,
Edward F. Moody,
Jacob Naylor,
Percival Roberts,
Wm. G. Rhoads,
J. H. Linville.

Minerals.

Clarence S. Bement,
Isaac H. Conrad,
Elias Wildman,
Dr. B. S. Howell,
Dr. F. A. Genth,
Theo. D. Rand,
Joseph Wilcox,
K. E. Griffith,
John C. Browne,
John C. Trautwine.

Models.

J. M. Wilson,
Wm. B. Bement,
Edward Brown,
Chas. H. Cramp,
Mordecai W. Haines,
Addison Hutton,
John Kile,
John Canby,
John L. Perkins,
S. Lloyd Wiegand.

Meteorology.

Dr. Chas. M. Cresson,
Dr. George Percival,
Dr. Henry Hartshorne,
Caleb S. Hallowell,
James A. Kirkpatrick,
Henry Morton,
James A. Meigs,
Pliny E. Chase,
James H. Cresson.

Meetings.

Henry Cartwright,
Enos Lewis,
Emile Geyelin,
Edward Longstreth,
Caleb S. Hallowell,
W. B. Le Van,
Coleman Sellers,
John Birkbeck,
B. C. Tilghman,
Henry Morton.

Scientific Proceedings.

Coleman Sellers,
John Birkbeck,
B. C. Tilghman,
Caleb S. Hallowell,
Henry Morton.

After this, the meeting was on motion, adjourned.

HENRY MORTON, *Secretary.*

Bibliographical Notices.

Modern Practice of the Electric Telegraph. A handbook for electricians and operators. By Frank L. Pope. Published by Russell Bros., 28, 30 and 32 Centre St., New York.

This little book of 128 pages, contains as much valuable information as it is possible to pack into so small a compass consistently with the use of good sized, well spaced, and thus pleasantly legible type, and abundant illustration.

Without attempting to enter into the historical or general theoretical discussion of the subject, it gives a full and clear account of the American system of telegraphy, and of the various new processes, by which defects in connection, leakage, and other faults are discovered and localized, and of the method of using the galvanometer and resistance coils in determining these and other points.

While in no way comparing with such a book as Sabines, considered as an exhaustive treatise on the theory and practice of telegraphing, it is an excellent supplement to the latter as exhibiting the distinctive points of American practice, and for the working telegrapher and student, is a more valuable, because a more attainable and more easily wielded tool. In mechanical execution, this book does credit to its publishers, and it deserves, in all respects, the success it will no doubt meet with, as filling a manifest want in the rapidly developing profession of telegraphy.

Treatise on the Power of Water as applied to drive flour mills, and to give motion to turbines and other hydrostatic engines, by Joseph Glynn, F. R. S. &c. Third edition. Published by D. Van Nostrand, N. Y. For sale by Claxton, Remsen & Haffelfinger, Philadelphia.

The quaintness perceptible in the above title might lead one to expect in this book, old and yet undeveloped theories and little practical information. Such, however, is not at all its character. A few pages of erudition as to antiquated and justly forgotten devices, and a few of elementary discussion as to the non "elementary" character of water and its true chemical composition and physical properties, dispose of this division, and we then find good and full descriptions of the various forms of water wheels and engines, with rules for calculating the power developed, and the proper proportion of parts.

Among the new things described are the air compressors employed at the Mont Cenis tunnel, and the rock boring machines there used, ventilated millstones, conical mills, and other devices of like interest.

The Mississippi Valley: its Physical Geography, including sketches of the Topography, Botany, Climate, Geology and Mineral resources, and the progress of development in Population and Material Wealth. By J. W. Foster, L.L.D. Published by S. C. Grigg & Co., Chicago. For sale by Remson, Claxton & Haffelfinger, Philadelphia.

When we add to the above title our testimony that this work, in 443 large pages, does discuss with such fulness as these limits admit, the various subjects enumerated, and that in solidity and excellence of mechanical execution, it does credit to the well-known firms whose names are connected with it, we have said much in its praise.

As a literary and scientific work, it well deserves a place in every library, on the same shelf with the other standard authorities on similar subjects, and will be of the greatest value to any one who, for business or pleasure, wishes to make himself acquainted with the character and resources of our great midland valley.

Editorial Correspondence.

Editor of the Journal of the Frnklin Institute:—

If the spirit of the article on "Pumping Engines," in your last number, had not been detractive and unjust, I might feel called upon to reply to that part of it which deals with the principles upon which my engine is constructed.

As it stands, I only ask room for the following statement, compiled from official reports, from which it may, perhaps, appear that something beside "*the pertinacity with which many patents are urged upon the public, and the combinations of men and money formed to press them,*" has operated to make the engine "*notorious.*"

Respectfully, HENRY R. WORTHINGTON.

New York, June, 1869.

Performance of Pumping Engines, taking total amount of coal used during the year, and reducing to common standard of number of pounds lifted 100 feet high with 100 pounds of coal:—

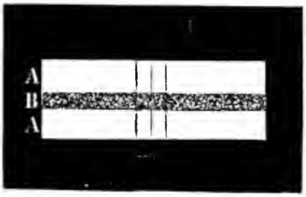
Delaware Works, from	Philadelphia	Report of	1869,	raised	168,834 lbs.
Germantown Works,	"	"	1869,	"	210,211 "
Chicago Crank Engine,		"	1865-66	"	302,520 "
Schuylkill Works,	"	"	1869,	"	383,857 "
24th Ward Works,	"	"	1869,	"	389,268 "
Jersey City, Cornish Engine,		"	1867,	"	436,078 "
Do. Do.		"	1868,	"	446,391 "
Charlestown, Worthington Engine,		"	1867,	"	455,617 "
Brooklyn, Direct Acting Engine,		"	1867,	"	458,849 "
Charlestown, Worthington Engine,		"	1868,	"	473,428 "

NOTE.—Of the above engines, all, except the Charlestown, were running from 18 to 24 hours per day.

During the year 1867, the Charlestown engine ran an average of $6\frac{6}{10}$ hours, and $8\frac{6}{10}$ hours during the year 1868.

SPECTRUM OF SUN-SPOTS.

The observations made by Mr. Lockyer, some time since, on this subject, and noticed by us, have been confirmed and extended by others lately published by Mr. Huggins. He observed that most of the solar lines were increased in width in the light from the umbra, as is indicated in the accompanying figure, where A and A represent the spectrum of the solar surface (a very minute portion of the entire spectrum being here included in the field), and B shows the corresponding spectrum of the light from the umbra. Such a result is attained by having the slit of the spectroscope so placed as to cross a spot and take light from the general surface at each side, then, as in the figure, we shall have a middle, less brilliant, spectrum produced by the light from the spot, and two others, one on each side, of greater brightness, because resulting from the light of the photosphere.



While most of the lines, as above stated, were *broader* in the spectrum of the umbra, it is worthy of note that C and F, characteristic of hydrogen, were not *stronger*.

The broadening of the lines is in accordance with all other indications, and is what we should be led to anticipate. Thus, as was explained in a previous notice, page 296, an increase in density of an ignited gas, so changes its power of specific emission, that it produces in the spectroscope *broad* luminous bands in place of narrow ones. It has been abundantly proved by experiment, that the absorptive and emissive powers are co-ordinate, hence, the ignited gas that in a rare state emitted and absorbed but individual rays, at a greater density would absorb, as well as emit, those of adjacent refrangibilities.

Sun-spots being unquestionably hollow, it follows, of necessity, that the absorbing atmosphere, drawn down into them, will acquire a greater density as it descends into the region of increased atmospheric pressure.

The fact that some lines, especially those indicating hydrogen, which we know to exist in the solar envelope from the testimony of the prominence and chromosphere spectrum, are not stronger in the spots than elsewhere, is not inconsistent with the above supposition; for, though there would be a greater depth of absorbing gas in the spots, this gas itself would be also more intensely self-luminous, and as the *strength* of the lines expresses only the *difference* between the absorption and emission as far as the observer is concerned, that is, in his direction; such a result as that observed might naturally follow. It must not be forgotten that the black lines, so-called, are by no means absolutely dark, but simply less bright than the neighboring regions of the spectrum.